THURSDAY, OCTOBER 20, 1892.

FRESNEL'S THEORY OF DOUBLE REFRACTION.

The Optical Indicatrix and the Transmission of Light in Crystals. By L. Fletcher. (Oxford University Press Warehouse, 1892.)

MR. FLETCHER has given us a valuable and most interesting book. He has attacked some parts of the theory of double refraction in a manner which is free from many of the objections that may be made to the method more usually adopted, and which has the advantage of closely resembling that by which Fresnel himself made his great discovery.

In dealing with this discovery we must carefully bear in mind two facts. The result of Fresnel's theory—the determination of the actual form of the wave-surface in a biaxal crystal—is undoubtedly true; the mechanical reasoning on which, in his second memoir on double refraction, he bases that result, is as undoubtedly false. In recent times the explanation of the properties of the wave-surface has been usually based on the erroneous mechanical reasoning. Mr. Fletcher, following Fresnel himself, has shown us how all these properties may be deduced from certain experimental results without the introduction of the false mechanics. In one sense this is a backward step, but since the advance had been in the wrong direction, it was necessary to retreat, and though Mr. Fletcher's book teaches us nothing new of the mechanism of double refraction, it puts the kinematics of the theory and the geometrical results which it entails on a sure foundation, and this is an achievement for which the author deserves our warmest thanks.

The history of the discovery of the law of double refraction, as given in Fresnel's own papers, is of the deepest interest. The form of the wave-surface, in a uniaxal crystal, a sphere and spheroid, had been discovered by Huyghens. Up to the time at which Fresnel wrote it was generally supposed that even in a biaxal crystal one ray obeyed the ordinary law of refraction. In 1816 Fresnel and Arago proved that in an isotropic medium the direction of the periodic disturbance to which light is due is transverse to the ray, and from that time Fresnel set himself to investigate the laws of double refraction. He soon saw that the reasoning which led him to expect that in a uniaxal crystal the velocity of one wave must be constant could not apply to crystals with two optic axes, and that in such, contrary to the usual belief, there could be no ordinary ray. This was announced in June 1820. The first memoir on double refraction, presented to the Academy November 19, 1821, but not printed till 1868, gives a detailed account of the experiments by which this result of his theory was verified, and contains the earliest development of the theory itself. According to Fresnel's views at this time the ether displacement in a crystal was at right angles to the ray—it is noteworthy that the latest development of the mechanical theory leads to this same result—and the velocity of propagation depended on the elasticity of the medium in the direction of displacement. Since, then, for any ray there are two possible velocities there must for that ray be two and only two directions of displacement, the light must be polarized in one of two directions. Since, also, the velocities are different in different directions, in Fresnel's view the elasticity corresponding to a given direction of vibration must depend on the direction, and a surface of elasticity can be drawn each radius of which shall be proportional to the elasticity in its own direction. When once this surface is known, the rest of the problem can be readily solved. Now, in the memoir we are referring to ("Premier Mémoire sur la Double Réfraction, Œuvres Complètes d'Augustin Fresnel," T. ii. No. xxxviii.) Fresnel proved, for the case in which the double refraction was not strong, for which therefore the distinction between the ray and the wave normal might be neglected, that if for a uniaxal crystal the surface of elasticity were a spheroid of revolution the wave surface and the laws of double refraction would be those discovered by Huyghens. It was an easy step from this to generalize and to suppose that in a biaxal crystal the surface of elasticity might be an ellipsoid, and to examine the results. This gave him at once, to the same degree of approximation as in the previous case, the form of the wave surface known by his name and the experimental laws of biaxial refraction discovered by Brewster and by Biot. As yet the result was only approximate; worked out more completely the theory can be shown to lead to the same form of the wave surface as that developed by Lord Rayleigh (*Phil. Mag.*, June, 1871). Huyghens' laws and those of Biot and Brewster are not accurately obeyed.

But Fresnel's mind moved rapidly, and a week later, November 26, 1821 ("Extrait d'un Mémoire sur la Double Réfraction," Œuvres, Vol. ii., No. xxxix.), he corrected his first results and announced his complete theory. The modifications required were not great; in his original theory he had supposed the displacement to be at right angles to the ray, in the final form it is supposed to be at right angles to the wave normal, *i.e.*, in the wave front, while the elasticity in any given direction, and therefore the corresponding velocity, is given by the reciprocal of the radius vector of a certain ellipsoid instead of by the radius vector itself. With these modifications the wave surface is accurately Fresnel's surface and the experimental laws of double refraction are accurately obeyed. To this ellipsoid (of which the inverse is Fresnel's surface of elasticity) Mr. Fletcher has given the name of the "Indicatrix." According to Fresnel's theory the two possible velocities of wave propagation in any direction are the reciprocals of the axes of the section of the indicatrix by the wave front. This law has been verified to a high degree of accuracy by direct measurement, and is thus made by Mr. Fletcher the basis of his treatment of the problem. These Memoirs of Fresnel's remained unpublished till 1868. The only printed announcement of the results was a notice in the *Moniteur*, December 12, 1821, with a view of claiming priority for the discovery, and the Report of the Referees, Fourier, Ampère, and Arago, who (August 19, 1822) recommended that it should be printed in full in the "Recueil des Savants Etrangers."

Instead, however, the Second Memoir on Double Refraction (Œuvres, tome ii., No. xlvii.) was printed

(tome vii. of the Recueil) in 1827, the year of his death, and this is the paper which contains Fresnel's latest developments of his theory. In it he suppresses entirely his method of generalization and develops that mechanical theory by which, to quote his biographer Verdet, "he endeavoured to rediscover truths which a profound intuition had first revealed to him." The truths remain, and though Fresnel's methods and their historical development are clearly given in his collected works and in M. Verdet's most admirable introduction to them, Mr. Fletcher has done good service to science in calling fresh attention to these earlier papers and in making a modification of this method of Fresnel's the foundation of his work.

In his note to the first memoir, when discussing the inexact reasoning by which Fresnel afterwards supported his mechanical theory, Verdet writes ("Œuvres de Fresnel," ii., p. 327):—

"Il pouvait sembler singulier que le résultat définitif d'un raisonnement incomplet et inexact en deux points fût une des lois de la nature dont l'expérience a le mieux confirmé la vérité. On a vu au contraire que cette loi s'était manifestée à Fresnel comme le résultat d'une généralisation toute semblable aux généralisations qui ont amené la plupart des grandes découvertes. Lors qu'il a voulu ensuite se rendre compte de la loi par une théorie mécanique il n'est pas étonnant qu'il ait, peutêtre à son insu, conduit cette théorie vers le but qu'il connaissait d'avance et qu'il ait été déterminé dans le choix des hypothèses auxiliaires moins par leur vraisemblance intrinsèque que par leur accord avec ce qu'il était en droit de considérer comme la vérité."

True though this may be, the fact remains that until Lord Kelvin developed his theory of a contractile ether a few years ago, no one of the distinguished men who have followed in Fresnel's steps had discovered a satisfactory mechanical basis for Fresnel's great generalization. But to return to the book before us. As has been mentioned, Mr. Fletcher's method of development differs somewhat from that indicated by Fresnel. He finds it more convenient to work with rays than with wave-normals or wave-fronts, and the construction he adopts is the following:—Draw a normal at any point of the ellipsoid of elasticity—the indicatrix in Mr. Fletcher's language. From the centre of the ellipsoid draw a line perpendicular to this normal, and consider a ray travelling in the direction of this perpendicular. Then the reciprocal of the intercept on the normal between the surface and the ray measures the velocity of propagation along the ray, and the plane of polarization of the ray touches the indicatrix at the point at which the normal is drawn. According to Fresnel's theory the radius vector drawn from the centre to this point is the direction of vibration in the ray, while according to the most recent modification of the theory, the motion takes place along the normal itself. From this simple construction the form of the wave-surface and all the known laws of the propagation of light in crystals are deduced in a strict and skilful manner. At the same time, while giving Mr. Fletcher the fullest credit for his originality, we are at times inclined to wish he had adhered more closely to Fresnel's method. He admits of course himself that a single ray cannot be propagated through the ether. We may hope that some of those who read his book will go on to study the mechanical theory of double refraction. Then they must deal with waves and not with rays, and they would find it an advantage to have had the one idea to guide them throughout. Again, the new method leads to a multiplicity of names for one and the same thing, and this is a disadvantage. We have ray-surface used for wave-surface, although the two are identical, nor is it easy at first to recognize the optic bi-normals and the optic bi-radials as the optic axes and the lines of single ray velocity respectively; but these are small points when compared with the main object of the book, which well deserves attention and careful study. The last chapter deals with the problem in a more general way, but space forbids us to follow Mr. Fletcher into the questions he there raises; it must suffice to call the reader's attention to it, and especially to the fallacy discussed in Section 17.

R. T. G.

THE PROGRESS OF HORTICULTURE.

Contributions to Horticultural Literature. By William Paul, F.L.S. (Waltham Cross, Herts: W. Paul and Son, 1892.)

FOR about half a century Mr. Paul has been labouring at the work of horticulture alike in the garden and at the desk. As a business man he has not confined himself simply to commercial routine. As an observer and an experimenter he has not been hedged in by the dogmas and prejudices of any particular school of science, and as a writer his aim has always been to record truthfully and instruct faithfully. It is a matter of congratulation, therefore, that the author should have gathered together in a convenient form some records of a lifetime's work.

Certain portions we should have eliminated as of past or of personal interest only; certain others as of relatively minor importance; but Mr. Paul is addressing a mixed audience with varied sympathies and interests, and it may be that the paragraphs we should mark for deletion would be those which others would best care to preserve.

Mr. Paul groups his writings, as here collected, under the three heads of (1) roses, (2) trees and plants, and (3) fruit culture and miscellanea. They would fall equally well under other categories, such as the commercial and practical, the æsthetic and the biological. In this notice we must confine ourselves to Mr. Paul's writings as a naturalist. Such, however, is the interdependence among various branches of inquiry, that it is almost impossible, in this connection to isolate any special subject, even if it were desirable to do so. From this point of view Mr. Paul's book is, though undesignedly, an apt representation of the present condition of horticulture. On the one hand, the relations of that art to the perception of and to the canons of beauty are obvious. Equally clear are its bearings on routine practice. On the other hand, its connection with biological science, in spite of the teaching and example of Darwin, is not yet adequately recognized; nor has the statesman as yet grasped the truth that progress in agriculture must follow to a large extent on the lines familiar to horticulturists. Of the many remedies proposed to mitigate and clear away the depression under which agriculture is suffering, none is more likely to be serviceable than the adoption, so far as

circumstances permit, of the principles and practice of the progressive gardener. This is very obvious to those conversant with the state of commercial horticulture, as contrasted with the condition of the corresponding department of agriculture, and it will be brought home to the thoughtful reader by the perusal of some of Mr. Paul's pages. It is interesting, too, to see that matters at which some minds would still be inclined to scoff as unpractical, or which they would regard as mere means of affording agreeable recreation, are the very departments in which the greatest practical successes have been achieved in the past, and which are of the best augury for progress in the future.

Biologically speaking, Mr. Paul has been not only a keen observer but a careful experimenter on a very large scale, and over a very long period. It is true his experiments have not been and could not have been made with the exact accuracy which we expect in the laboratory, but they have been made under conditions far more akin to those which occur in nature. Moreover, they have been made, although with a definite aim, yet without reference to any particular theory. The reader will accordingly find in these pages records of work and inferences from carefully planned experiments directly bearing on many subjects now attracting the attention of naturalists, such as hereditary transmission, variation from seed or from bud, selection, fixation, close fertilization, and the various degrees of cross-impregnation. Incidentally these subjects receive illustration in many chapters of Mr. Paul's book; but the address on "The Improvement of Plants," which was read in 1869 before the provincial meeting of the Royal Horticultural Society at Manchester, contains a summary of Mr. Paul's views on these subjects, which we strongly commend alike to the notice of naturalists and of agriculturists.

It is very interesting to compare what he says about selection and variation in plants, such as the Camellia, the Chinese Primrose, or the Hollyhock, which are the offspring of what we regard as pure species, with the corresponding processes in the Rose, the Pelargonium, or the Chrysanthemum, which are veritable mongrels. In this connection we may in passing allude to the power which the gardener has, of course within limitations, of creating new forms. The orchid cultivator, for example, inferred parentage of certain hybrids met with in a wild state, but he has since proved the correctness of his inference by actually producing in his orchid-house many of the same forms that occur in the forests of the tropics. Another very striking case (not specially alluded to by Mr. Paul) is the production and development of what are known as tuberous Begonias. These have been evolved by the art and patience of the gardener within the last quarter of a century from repeated crossing between certain Andean species of Begonia and their descendants. The result is the establishment of a race so totally distinct from anything yet known in nature as would justify a systematic botanist in forming a separate genus for their reception. Many an accepted genus is based upon less important points of distinction than those which characterize the tuberous Begonias, and which, indeed, have been gathered together by Fournier under the genus Lemoinea. The degree of permanence of this artificially formed genus is, of course, unknown; but we do know already that the peculiarities are reproduced from seed, and that each year the plants are, as the gardeners say, becoming more "fixed." We have alluded to these as illustrations of the kind of work upon which Mr. Paul has been engaged for half a century. They may be taken as examples of the material he has gathered together in this book, which is not merely presented for the delectation of the ordinary lover of flowers or the profit of trading horticulturists, but is also calculated to increase the productive resources of the country, as well as to forward the progressive development of our knowledge of the natural history of plants.

As a further illustration of Mr. Paul's method we cite in conclusion a passage which will, we think, justify us for recommending to scientific readers the perusal of a book which they might be disposed, from its title, to think had little in it to interest them. "My experience in selecting, hybridizing, and cross-breeding tells me that he who is seeking to improve any class of plants should watch narrowly and seize with alacrity any deviation from the fixed character, and the wider the deviation the greater are the chances of an important issue. However unpromising in appearance at the outset, he knows not what issues may lie concealed in a variation, sport, hybrid, or cross-bred, or what the ground newly broken is capable of yielding under careful and assiduous cultivation. If we would succeed in this field we must observe, and think, and work. Observation and experiment are the only true sources of knowledge in nature, and while observing and experimenting we should above all things guard against prejudices."

MAXWELL T. MASTERS.

LIFE IN MOTION.

Life in Motion; or, Muscle and Nerve. By John Gray McKendrick, M.D., F.R.S. (Adam and Charles Black, 1892.)

UNDER this title Prof. McKendrick gives us the gist of six lectures delivered by him during last Christmas holidays to a juvenile audience at the Royal Institution of Great Britain; and, judging from this little work, it is evident that no pains was spared by him to render these lectures as instructive and interesting as abundant illustrations and experiments could make them. In presenting these lectures to the public in book form he places us under an obligation gratefully to be acknowledged, for professional physiologists stand alone amongst their colleagues in other departments of science in their disdain of any attempt at the production of attractive and simple scientific literature. In very pleasing sympathetic style the reader is introduced to the world of motion and to the special motions of the living muscle. He is shown how the movements of a muscle are recorded by the physiologist, and the apparatus used for its stimulation. Artificial tetanus is described, the muscle sound and its elasticity referred to, and a perhaps too short description given of amœboid and ciliary motion. The physiology of the nerve is then discussed, and the production of heat in muscle. In the fifth lecture is a short account of the sources of muscular energy, a comparison is drawn between a muscle and the steam-engine, and a comparatively detailed account of muscle fatigue is given.

The sixth and final lecture deals with the electrical phenomena of muscle and with a very curious group of fishes termed "electrical."

The arrangement of the book is excellent, yet we are inclined to think that it shares with many other works on physiology one common fault. What we all want to know more about is the life and activity of the organism, and the physiologist very rightly spends much of his time in experimenting in every conceivable way, and generally with isolated parts of the organism. His apparatus is often of the most varied and intricate kind, and his experiments yield him definite results. Many of these results, however, are at present of little value in shedding light on physiological processes, and should not, we think, obtain the prominent position they now occupy in the text-books. To take an example, the experiment to demonstrate the muscle curve, in which the muscle is isolated and stimulated electrically, is one of the stock experiments minutely described in every text-book. In this experiment the muscle is separated from its antagonistic muscles, stimulated in quite an unnatural way, and the result of the experiment is totally different from what takes place in a contracting limb. It is certain that in nearly every text-book the reader will find that from this and similar experiments he is apt to obtain incorrect and misleading ideas. He no doubt learns something regarding very interesting electrical machinery, but very little physiology. Of recent years far more attention has been bestowed upon the movements of muscles in the limbs, and comparative physiology is at last asserting its influence. It is to be hoped that when this knowledge finds a more prominent place in text-book literature, "muscle and nerve physiology," in the proper sense of the term, will be more satisfactorily taught.

Returning to what more exclusively concerns Prof. McKendrick's book, we may point out a slip on page 81, where it would appear that the muscle sound corresponds in pitch to the fundamental tone of a body vibrating 19·5 times a second, instead of to one vibrating at twice that rate, and that Prof. McKendrick does not interpret this sound on the lines followed by Helmholtz and others. On page 91 the modern view of a "cell" is represented in a drawing, and the nucleus has inadvertently been omitted. On page 31 a long and short circuiting key is represented, while a simple key is described in the accompanying text. These, however, are but trivial faults to find in an excellent little work, which is most admirably got up and beautifully illustrated by nearly one hundred excellent figures.

The reader will, we think, obtain a good insight into a department of physiology, and will be stimulated to further research in the literature of this interesting subject.

J. B. H.

PLUMBING.

Principles and Practice of Plumbing. By S. Stevens Hellyer. (London: George Bell and Sons, 1891.)

THOSE who are acquainted with Mr. Hellyer's larger book on domestic Sanitation, "Dulce Domum," will not find much new matter in the present volume, but the subjects are treated less discursively, and are fairly well brought down to date.

There is no trade which has been more discussed in recent years than that of plumbing, and if plumbers are not impressed with a sense of their responsibilities, it is certainly not the fault of the architects and engineers who employ them. The manual skill necessary to perform the most ordinary operations is in itself so difficult that many workmen fail to acquire it; and, on the other hand, many experts in the details of the craft are never properly educated in the principles of sanitation which are necessary to make their work effectual from a sanitary standpoint. It is the combination of both kinds of knowledge in the writer which makes Mr. Hellyer's books of exceptional value. It matters little whether it is an architect on one hand, or a working plumber on the other, who studies them, because they are of equal value and of equal interest to both. The present handbook is specially valuable in these respects because most of the information upon matters of practical workmanship is given concurrently with the reasons which should control the details and the principles which should be in evidence when the work is finished.

No one unacquainted with the practical difficulties which frequently crop up in sanitary practice can realise how much knowledge and experience is necessary to overcome them. Houses in London often present the most puzzling problems, and an intimate acquaintance not only with the principles and practice of the subject, but also with all the most recent appliances, is required for their successful solution. The ventilation of all the different parts of a complicated drainage system, including that which is necessary to prevent the syphonage of traps, sometimes requires an amount of thought and attention which a layman would think was uncalled for in the face of its apparent simplicity. It is no wonder that there are frequently failures to meet the highest standard of excellence, especially when incompetent persons are employed to design and superintend the necessary operations. On the other hand, there are thousands of houses in London in which no such difficulties occur, and in which the drainage and plumbing arrangements ought not only to be extremely simple in themselves, but intelligible to the ordinary householder. When such cases are entrusted to a builder, or an intelligent plumber, the first requisite is the manual skill required to carry out the various details, and this must be acquired by the workman through apprenticeship, or from his having acted as the assistant or "mate" of a journeyman for several years. The next requisite is that he should have a clear knowledge of what he is going to do and why he does it. This may be acquired to a great extent from his being familiar, in his capacity as a workman, with the designs of an architect or engineer under whose directions he has been employed, and it is to such men that Mr. Hellyer's text-book should be specially valuable. By studying its pages he will avoid many mistakes. He will know what sort of joint to make, what kind of trap to avoid, how to secure the traps from syphonage, and how generally to complete his work so as to pass the latest standards of excellence. We can equally recommend it as a text-book for architects

who, although they are unable to acquire any technical skill in carrying out the operations themselves, should have, nevertheless, an intimate knowledge of the principles which they ought to embody.

We think that more space should have been devoted to that portion of the book which deals with drainage proper. While nearly 100 pages are given to lead-laying and the jointing and bending of pipes, only about twenty pages are devoted to house-drains, and a great part of this is occupied with illustrations of appliances. Not more than one or two pages are given to the subject of cast-iron drains, although they are strongly recommended, and the subject is a very important one. We trust, however, that the author will remedy these deficiencies in the future editions which will doubtless be required to supply the demand for his excellent text-book.

OUR BOOK SHELF.

A Lecture Course of Elementary Chemistry. By H. T. Lilley, M.A. (London: Simpkin, Marshall, Hamilton, Kent and Co., 1892.)

THE abrupt use of chemical terms, and the condensed style adopted by the author in this book, make it evident that it is not specially designed to smooth down the difficulties which confront the unaided learner who approaches chemistry for the first time. It seems rather to be fitted to replace the notes which might be taken on a course of lecture instruction. Regarded in this light it is a useful volume, the knowledge it contains being, in the main, sound and to the point.

It deals with the metals as well as with the non-metals, and dovetailed with the ordinary chemical information are many instances that the author has tried to keep pace with current work, and has attempted to give the student all the important points to be noted in a fairly complete course of elementary chemistry.

A short series of exercises chiefly in chemical arithmetic are given at the end of the book, and a table of contents and an index are supplied.

It would be advisable on p. 53 to say that ordinary sulphur crystallises in the rhombic system. To speak of the crystalline form as an octohedron tends to create an impression common among students, that ordinary sulphur belongs to the cubic system. Fluorine was not made by the electrolysis of liquefied hydrofluoric acid, but of a solution of potassium fluoride in the acid; the pure acid is a non-electrolyte.

It is hardly correct to state that calcium sulphate and hydroxide are the only known examples of solids less soluble in hot than in cold water; calcium isobutyrate and one of the thorium sulphates are additional instances. On p. 98 the flame colorations of potassium and sodium are confused, and brass seems to be omitted in treating of the alloys of copper and zinc. J. W. R.

Longmans' School Geography for North America. By George G. Chisholm and C. H. Leete. (New York: Longmans, Green, and Co.).

IF Mr. Chisholm's well-known geographical text-book was to be extensively used in the United States, it was inevitable that it should be altered in a way which would adapt it to the special needs of American schools. The task was undertaken by Mr. Leete, and he has accomplished it with much skill and judgment. The parts he has rewritten are those relating to America in general, North America, and the United States. To these he gives a prominence which was not necessary or desirable for European students of geography, but which is no doubt essential for learners on the other side of the Atlantic. The plan of Mr. Chisholm's book and the spirit of its execution have both been maintained, and the work ought now to be quite as useful in the New World as it has already been in the Old.

Garden Design and Architects' Gardens. By W. Robinson, F.L.S. (London: John Murray, 1892.)

THE author of this book is firmly convinced that to clip and align trees in order that they may "harmonise" with architecture is "barbarous, needless, and inartistic." He is in love with Nature's methods, and would give them in gardens much freer scope than is accorded to them by persons who like best a certain trimness and formality. It is to be regretted, perhaps, that Mr. Robinson deals with the subject in so polemical a temper, but the cause for which he contends is good, and he does excellent service by bringing out prominently what has always been the essential principle of the best and most characteristic kind of English landscape gardening. The value of the essay is greatly increased by a number of well-selected illustrations.

LETTERS TO THE EDITOR.

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The Alleged "Aggressive Mimicry" of *Volucellæ*.

IN the course of a review (NATURE, October 6, 1892, p. 535) of a book, "Animal Coloration," by Mr. Beddard, Mr. Poulton takes occasion to refer to a theory professing to elucidate the resemblance of *Volucellæ* to humble-bees, &c. This reference is occasioned by the suggestion of a counter-hypothesis by Mr. Beddard. The view adopted by Mr. Poulton ("Colours of Animals," 1890, p. 267) is that proposed by Kirby and Spence, and subsequently alluded to by Künckel d'Herculais ("Organ. et Dével. des Volucelles," Paris, 1875) and others; but as Mr. Poulton makes no reference to these authorities he may be assumed to accept the full responsibility. In the place named he says:—"The boldness of these enemies sometimes depends on the perfection of their disguise. Thus the larvæ of flies of the genus *Volucella* live upon the larvæ of bees and wasps. *Volucella bombylans* occurs in two varieties, which prey upon the humble-bees *Bombus muscorum* and *B. lapidarius*, and are respectively like these Hymenoptera. The resemblance is very perfect, and the flies enter the nests to lay their eggs." Mr. Beddard (*l. c.*, p. 225) criticizes the view that the fly resembles the bee that it may with impunity enter the nest, and proposes to look on the presence of the fly's larvæ in the bees' nests as akin to the presence of supposed "pets" in the nests of ants. As Poulton points out, this suggestion leaves the original difficulty of the likeness of the fly to the bee untouched.

Having little interest in either of these speculations, which seem fantastic and premature, it is with reluctance that I take part in the discussion. The case, however, of *V. bombylans* is not only interesting as a striking, and to us in England a most accessible instance of the phenomenon of Mimicry, but as an example of Variation it is almost unique among animals, while among plants perhaps it is paralleled only by Darwin's famous case of the peach and the nectarine. It is besides a case well suited for experiment and close observation. The nests of surface building bees may towards evening be lifted bodily, bees, *Volucellæ*, and all, with a spit of earth, and transferred to a box. This may be taken home and set next morning on a window-sill, when on opening the box the bees will go on with their work for the rest of the summer. If any one seeks an opportunity of honestly trying to get to the bottom of a case of Mimicry, instead of speculating about it at large, he can scarcely find a better case than this. The need for such observations is great, for the account confidently given by Poulton, though according well with his hypotheses, accords with the truth less well.

In these circumstances it may not be out of place to give a brief statement of the facts as they were established by entomologists long ago. The *Volucellæ* are a small group of flies, con-

taining four British species (Verrall, "Cat. Brit. Dipt.," 1888); of these most if not all resemble various Hymenoptera. The commonest and most remarkable is *V. bombylans*, which may be seen in any English hedgerow on a sunny day in early summer. This fly exhibits the rare condition of existing in two distinct forms in both sexes. The one form is black with a red-tail, in no small degree resembling a small worker of a red-tailed humble-bee. such as *B. lapidarius* L. or *B. Derhamellus* Kirb. The other form has a yellow border to the thorax, yellow hairs on the antero-lateral parts of the abdomen, and a grey tail, to an equal degree resembling a small worker of one of the several yellow-banded humble-bees, *e.g. B. hortorum* L., *B. terrestris* L., or *B. Scrimshiranus* Kirb. Both varieties occur in both sexes and are about equally common. The problem of the evolution of these distinct forms is thus one of the most complex. Some may ask, If the varieties are thus distinct, how are they known to be one species? The evidence of this is (1) that no point of structure can be found to differentiate them, (2) that males of the one variety have been seen coupled with females of the other and *vice versa* (Macquart, "Suites à Buff.," p. 479; Zeller, *Stet. ent. Ztg.*, 1842, p. 66), and lastly (3) that intermediate forms have been found as rarities (Erichson, *Stet. ent. Ztg.*, 1842, p. 115). This evidence may not satisfy all, but as regards Mr. Poulton the identity of the two as one species is not in dispute, for he admits this.

But though the likeness of *V. bombylans* L. and its var. *mystacea* L. (= *plumata* de Geer) to the red-tailed humble-bees and to the yellow-banded humble-bees respectively, is really close, neither these forms nor the less common var. *hæmorrhoidalis* Zt. present any special likeness to *B. muscorum* L., which has a bright brown thorax and a grey abdomen. It is true that Künckel has spoken of a resemblance between the var. *mystacea* and *B. muscorum*, but it is hard to see upon what ground, for indeed it is much as if one were to liken a tabby cat to a fox. As Künckel himself says, the great resemblance of the fly is to the yellow-banded *B. hortorum*.

To return to Mr. Poulton's statement, he says that the two varieties prey upon "*Bombus muscorum* and *B. lapidarius*, and are respectively like these Hymenoptera." These words contain an ambiguity which I cannot believe intentional. But supposing for a moment that one of the varieties *were* like *B. muscorum* (which it is not), the sentence must be taken to mean that each variety preys upon the species of bee which it most resembles, the red-tailed variety on the red-tailed bee and the yellow variety on the other. This is indeed demanded by the hypothesis of "Aggressive Mimicry." In this form the statement is often made, though I never met it elsewhere in print. I invite Mr. Poulton to produce observations in support of that statement. If he will establish it he will do a useful work. When this statement was written I must believe that Mr. Poulton had not read the several authorities on the subject, many of whom relate how both varieties have been reared from the nests of each type of bee, both from the red-tailed and from the yellow-banded (Künckel, p. 58; Drewsen, *Stet. ent. Ztg.*, 1847, p. 211; F. Boie, Kröyer's "Naturh. Tids.," 1838, p. 237). It is still possible that both varieties are born of one mother, and it is possible, too, that each female does her best to choose the nest of a bee like herself, but in support of this hypothesis I know no evidence; and indeed Künckel (p. 58), after considering this possibility, gives it as his opinion that probably the varieties of *V. bombylans* lay indifferently in the nests of all *Bombi*. From the omission of these facts, which to an appreciation of the evidence are vital, we should infer that Mr. Poulton was not acquainted with Künckel's work, were it not that he repeats Künckel's selection of *B. muscorum* as a form resembled by one of the two varieties.

But though Mr. Poulton is wrong in saying that either variety specially resembles *B. muscorum*, he is right in saying that *V. bombylans* preys on this bee's nests, for *both* varieties have been bred from them, even from the same nest (Künckel, p. 58). In my rooms at this moment are several nests of *B. muscorum*, each containing many larvæ of *V. bombylans*, resting for the winter, to emerge in summer, as I hope.

There is then evidence that the two varieties, though they may breed together, yet remain substantially distinct; and that though they respectively resemble different species of bees, they are both found together, not only in nests of bees which they resemble, but also, and in my own experience, more abundantly, in the nests of another bee which they do not resemble.

Mr. Poulton further omits to mention that *V. pellucens*, though in nowise resembling the common wasp, yet lives in its nests, together with *V. inanis* which does resemble a wasp, and *V. zonaria* which is like a hornet (Künckel, pp. 54 and 55). This fact also I commend to Mr. Poulton's ingenuity.

The publication of statements like this of Mr. Poulton's, omitting most salient facts—facts, besides, which, though adverse to his speculations, add a ten-fold interest to the subject—is surely unfortunate. It may be replied that Mr. Poulton's book is of a popular character and does not aim at the completeness of scientific work; but in making choice of evidence, even for popular exposition, it is well to remember that the value of facts is not to be measured by the ease with which they may be momentarily fitted to the sustenance of a facile hypothesis.

William Bateson.

St. John's College, Cambridge, October 9.

Induction and Deduction.

Miss Jones agitates a question that ought not to settle down without having caused that discussion which its propounding is fit to awaken.

This discussion does not, however, relate to the mutual relations of Induction and Deduction—at least, not as the main topic thereof. It relates to the fragmentary condition of that which is usually referred to and accepted as logic. We are so apt to take it for granted that our so-called logic is tolerably competent and complete as an account of human reasoning in general, that it is of great utility when some one—as Miss Jones does now—raises a question that is adapted to direct our reflections towards some one of the several, perhaps many, gaps that exist in that most important, but too often not understood and misunderstood, branch of science. It is with this specially in view that I have ventured to write this.

In geometry nothing is more usual than to draw a universal conclusion from a case that, to all ordinary ways of apprehension, seems to be a single instance. Indeed, this is one of the cardinal features of geometrical reasoning. Perhaps we might well say that it is the most characteristic feature. It is this feature that the question that Miss Jones agitates ought to call into prominent notice.

She selects for her purpose the case of the isosceles triangle, and asks, How, from premising that the angles at the base of an (one) isosceles triangle are equal to each other, are we logically warranted in concluding that that same equality is true of all isosceles triangles?

That we do thus conclude is known to all, as is also the truth that such a conclusion is a typical one in geometry. Nor have we, nor can we have, the least misgiving as to the rigorous validity of such conclusions.

It would be a digression for me to point out here the essential characteristics of that form of reasoning which, properly speaking, is induction. It is sufficient for my present purpose to remark that true induction is utterly unable to yield us any conclusion that is more than *probable and approximate*.

From these characteristics alone we may know that our geometrical conclusions in view are *not*, as Miss Jones takes them to be, inductive conclusions.

But since our geometrical conclusions are natural and valid, the question still remains, What sort of conclusions are they?

If we propose to call them deductive conclusions, then, when we revert to the array of syllogisms, categorical, hypothetical, disjunctive, dilemmatic, &c., we find none of them, nor any combination of them, that can by any means be made applicable. We have to get not merely from an apparently particular but from an apparently absolutely singular proposition to a universal one. To do this deductively, the body of doctrines and canons, that is usually called logic, confesses itself wholly unable. It lays down as one of its cardinal rules, one that it declares is "founded upon the Laws of Thought," that if any premise is particular, then only a particular conclusion can be drawn.

Nevertheless, I am going to submit that the reasoning under discussion is a true instance, not of induction, but of deduction. I submit that the reasoning that we do actually follow is that which may be formulated thus:—

This isosceles triangle is ANY *isosceles triangle.*

The angles at the base of this isosceles triangle are equal to

each other. Therefore, the angles at the base of any (or every or all) isosceles triangle are equal to each other.

In order to make the nature of this reasoning plainer, I will put the same in symbols.

Put X = the isosceles triangle in general;
Z = this particular isosceles triangle;
a = angles at the base;
e = equal to each other.

Then our reasoning will appear thus:—

Z is X,
a of Z is e;
∴ a of X is e.

This looks very much as though we had in hand a case of the logic of relatives.

We will all recollect the challenge of De Morgan:—"If any one will by ordinary syllogism prove that because every man is an animal, therefore every head of a man is a head of an animal, I shall be ready——to set him another question." This would be in symbols—

All M is A;
∴ h of M is h of A.

Our case, according to this sort of formulation, would appear—

Z is any X;
∴ a of Z is a of X,
a of Z is e;
∴ a of X is e.

The distinguishing characteristic of our case as compared with the case put by De Morgan resides in the different natures of the two propositions—

All M is A,
and Z is any X.

The former is the usual universal categorical affirmative proposition of ordinary logic. The latter is a sort of universal categorical affirmative proposition that certainly exists and is important, but which has not yet been recognized, unless it may be by the quantifiers of the predicate in their proposition—

All A is all B.

It implies rigorously that not only

Z is any X,
but that Z is every X,
Z is all X,
any (or every or all) X is Z
,, (,, ,,) not X is not Z
,, (,, ,,) ,, Z ,, X
any X is every Z
every X is any Z
every not X is any not Z
any ,, ,, ,,
&c., &c.

In truth, to a superficial notice, it may easily seem to confuse the most important logical distinctions. But this is only because we are so used to identifying logic in general with the logic of extension. It is the logic of extension, or in other words, metric logic, that has been persistently tendered to us as the only logic worthy of study, if not indeed as the only logic practicable or perhaps possible. Yet we can now, I think, see that geometry at least makes great use of a logic that is not the logic of extension, and that the existence of geometry is an earnest that that other logic may be developed and formulated, if not completely at least to some very useful extent.

The proposition,

Z is any X,

is, as I conceive, a proposition of the logic of intension. It applies not to things, or to concepts in connection with things, but to pure abstract concepts like geometrical figures, whose marks are exhaustively specified, or, if any are not specified, the same depend upon and are implied by those that are specified.

One point more remains to be explained.

We must not from the recognition that Z is any X, and the rigorously following proposition that whatever is true of either X or Z is true of the other, conclude that these propositions should if valid hold for marks that are accidental to Z, or to any single instance of X. If we fail to keep clearly in mind the intensive scope of our propositions, we may discredit them, or one of them, by observing that although we have laid down that whatever is true of Z is true of any X, yet nevertheless it does not follow that Z is any X, as regards say the size of Z or any single instance of X. Size may not be any necessary mark of either, and if so it is for all logical purposes impertinent to the propositions in question, and must be altogether ignored.

I will conclude by saying that the inference actually made in the case put by Miss Jones is a deduction, because it necessarily follows from the premises laid down. Logic has no connection with the truth of premises; it only says what certain propositions entail. If in an intensive sense this isosceles triangle is any isosceles triangle, then any isosceles triangle is this isosceles triangle, and every isosceles triangle is this isosceles triangle, and all isosceles triangles are this isosceles triangle, and if the angles at the base of this isosceles triangle are equal to each other, it follows necessarily that the angles at the base of all isosceles triangles are equal to each other.

Chicago, August 16. FRANCIS C. RUSSELL.

BY the courtesy of the Editor of NATURE I have been allowed to read Mr. Francis C. Russell's very interesting remarks with reference to my note on "Induction and Deduction" in NATURE of July 28 (p. 293).

I agree with Mr. Russell as to the validity and certainty of an inference from equality of angles in *one* isosceles triangle to equality of angles in *all* isosceles triangles. But while I regard this inference as a "true induction" because it is an inference to a general proposition on the strength of a particular instance, Mr. Russell denies that it is an induction because he holds that induction can give only approximate and probable conclusions, and considers that the certainty which he allows to belong to the geometrical conclusion in question is due to the fact that the inference is *not* from a particular instance, but is really and truly *from* universal *to* universal—the one isosceles triangle from which the argument starts being a kind of "pure abstract concept," so that we can say—

This one isosceles triangle is *any* isosceles triangle; therefore *every* isosceles triangle is *this* isosceles triangle, &c.

This appears to me to be entirely inadmissible. How can *this* triangle *BE that* and *the other* triangle? To say that it is, is to lose sight of the distinction between *identity of individuality*, and *similarity of characteristics*. And that the assertion (*this* triangle is *every* triangle) is untenable appears also from Mr. Russell's own admission further on, when he says that "we must not, from the recognition that Z is any X, and the rigorously following proposition, that *whatever* is true of X or Z is true of the other, conclude that these propositions should, if valid, hold for marks that are accidental to Z or to any single instance of X." If Z *is* any X, how can any X have marks which Z has not, or Z have marks which any X has not? We cannot get out of the difficulty by reference to extension and intension, for this reason, that every categorical proposition, to be significant, must be read both in "intension" and in "extension"—that is, affirmatives must be understood as asserting identity of extension (application) in diversity of intension (signification), while negatives *deny* identity. "This isosceles triangle is *any* isosceles triangle" can have a useful signification only if it is interpreted to mean—

This triangle [not *is* but] *is similar* (in so far as isosceles) *to* any isosceles triangle—that is, all are *similar* in respect of the characteristics which are inseparable from equality of sides. Hence (as I said in my letter, July 28) "in all cases equality of angles at the base is inseparable from equality of sides."

I am not clear what precise meaning can be attached to the expression "pure abstract concept," still less how a geometrical *figure* can be an abstract concept. I am, moreover, disappointed that Mr. Russell makes no examination whatever of my own attempt to formulate the process from Particular to General.

With reference to Mr. Russell's symbolical argument—

Z is X,
a of Z is e,
∴ a of X is e,

I think that it may be logically described as a process either (1) of Substitution (Jevons)—a kind of Immediate Inference dependent on identity of application—thus:—

Z is X;

∴ X may be substituted for Z.

In a of Z is e
substitute X for Z,
and we have a of X is e.

Or (2) a combination of what Jevons calls Immediate Inference by Complex Conception (which I should like to class with some other Immediate Inferences as Extraversion, which is largely used in mathematics) and Mediate Inference; thus—

Z is X . . . . (*a*)
∴ a of Z is a of X . . . . . . (*b*)
But a of Z is e . . . . (*c*)
∴ a of X is e . . . . (*d*)

(*b*) is Inference by Complex Conception from (*a*); (*b*) and (*c*) are the premisses which give (*d*) as their (syllogistic) conclusion.

Cambridge, October 11. E. E. CONSTANCE JONES.

The Temperature of the Human Body.

MR. CUMMING's second or "physical" query will, I think, require no answer if his first or "physiological" question is replied to. If an isolated muscle from which evaporation was prevented could go on working in a heat enclosure, and always remain at a lower temperature than the enclosure (which it could only do by transferring heat from itself to its surroundings), we should have to ask in good earnest how this was consistent with the Second Law of Thermodynamics. We are quite certain, however, that the temperature of the working muscle would always, when a steady state of things had been reached, be above that of the enclosure.

The temperature of an isolated muscle during activity (assuming that it could be kept alive and evaporation prevented) would, of course, not only be very much higher "at the equator" than "at the pole," but also somewhat above that of the surrounding bodies in either latitude. The intact homoiothermal animal, even when the temperature of the air is greater than that of its blood, is on the whole, within the limits which can be borne, always losing more heat to its surroundings than it receives from them. For heat is still becoming latent at the evaporating surfaces of the body, the skin and the respiratory mucous membrane, even when the balance of gain and loss by radiation, &c., is telling the other way; and, indeed, in general more evaporation than usual is going on when this is the case. The temperature of these surfaces is always kept below that of the blood which comes to them. The blood, therefore, always loses heat here, and gains it from the muscles, which accordingly transfer heat to a medium colder than themselves, even when the external temperature is higher than that of either.

If, of two similar and similarly situated men, A and B (I ask pardon for degrading an austere geometrical phrase to such loose and vulgar application), exposed to the same high temperature (above that of the blood, say), A sweats little and B much, while the blood-temperature of both remains constant, A must either produce less heat than B or lose more in other ways than evaporation of sweat. He may produce less either because he works less than B, or because even at rest his metabolism is not so active. Or an extra loss of water-vapour from the lungs may make up for the diminished loss from the skin. For example, in the dog, which has but few sweat-glands, nearly the whole of the evaporation takes place in the respiratory tract. Of course much water is evaporated from the skin which never appears as visible sweat; and it is possible that some persons give off a greater proportion of the total perspiration in this way than others do, the quiet steady sweater, if one may be allowed the expression, getting through as much work on the whole as the steaming paroxysmal kind of fellow who breaks out into dewdrops on the smallest provocation. But it should be clearly recognized that an air temperature equal to or above that of the blood is occasional, and not permanent in any latitude, and that men, and even animals, adopt expedients to avoid such extremes and to tide them over.

Any good recent text-book of physiology will give the information asked for as to what is known of the mechanism by which the temperature of warm-blooded animals is kept approximately constant. It is too wide a subject to be entered into here. In man the regulation of the heat loss seems to be far more important than any regulation of the production of heat. The former is, of course, largely voluntary, but the quantity of blood going through the skin, an important factor in more than one way, is greatly influenced by reflex nervous impulses. It is doubtful whether the very considerable heat capacity of the bodies of large animals has been sufficiently taken into account in its bearing on the steadiness of the blood temperature. This in itself prevents any *sudden* change. In some animals, and apparently more especially in small animals—*e.g.*, the rabbit and guinea-pig—the production of heat, as well as the loss, is very distinctly under the control of the nervous system, and is increased when the external temperature is lowered, and diminished when it is raised.

Of course, as your correspondent is doubtless aware, we do not really know what kind of a machine a muscle is, except that it is a machine by means of which the potential energy of the food is partly transformed into mechanical work and to a much greater extent into heat. Up to a certain limit the work and the heat *increase together*, although less heat is given off by an active muscle which is allowed on the whole to do external work than by the same muscle when it constantly undoes its own work.

G. N. STEWART.

New Museums, Cambridge, October 11.

THE following brief account of the working of the heat mechanism of the human body will, I hope, help to make clear to Mr. Cumming the problems of which he seeks the explanation.

The temperature of a man at the equator is within a degree Centigrade the same as that in the arctic regions. This is because, in the first place, in the arctic regions the loss of heat from the body is very slight, and in the tropics it is very great, for (*a*) in the tropics more perspiration is secreted by the skin, and this, in consequence of the high temperature of the air, evaporates very quickly, and hence the body is kept cool. It is true, as Mr. Cumming says, that in the tropics people may not be observed to perspire freely, but that is simply because as fast as the perspiration is secreted it is evaporated. It is what is called insensible perspiration. (*b*) More water is secreted by the bronchial mucous membranes in the tropics, and in consequence of the higher temperature of the air it, like the perspiration, evaporates very quickly. The excessive secretion of moisture by the body when the temperature of the air is high, is shown in a Turkish bath, and leads, in a bath of about two hours' duration, to a loss of weight amounting with some persons to three pounds, and to a great diminution in the quantity of urine secreted. (*c*) In the tropics the vessels of the skin are more widely dilated than in the arctic regions, hence there is more blood in it, and therefore heat is more readily radiated and conducted from the skin to the external atmosphere. (*d*) The specific heat of the body is very high, and so it cools very slowly in the arctic regions. Judging from some experiments I have made on animals, it is, at the usual temperature of the human body, well over 1·0. (*e*) The above facts are certain, but in addition, for all we know to the contrary, the skin may, under different conditions, have different radiating powers quite apart from the quantity of blood in it.

In the second place, although it has not been calorimetrically proved, it is very highly probable that in the arctic regions the quantity of heat produced by the body is much greater than in the tropics.

With regard to the second query of Mr. Cumming, no doubt, as he says, the human body in the tropics must often be the coolest of surrounding objects; in this case it cannot lose anything by radiation or conduction, but it is kept cool by the rapid evaporation of perspiration (usually insensible) and fluid secreted by the bronchial mucous membrane. Whether or not a man in the tropics produces any heat under such circumstances has not been demonstrated, but probably, although the production of heat falls very low, it does not entirely cease.

65 Harley-street, W. W. HALE WHITE.

Photographic Dry Plates.

I HAVE found great difficulty in obtaining fresh photographic dry plates of whatever maker, from dealers, who frequently pass off upon the purchasers packets of plates which have been in stock for a long time, and consequently unfit for use. It has therefore occurred to me that this trouble might be avoided by the makers dating every packet as issued by them, thus following the custom of the Platinotype Company with their tins of paper. By such a system the purchaser would be able to protect himself,

and many makers' plates would be found much more satisfactory.

I shall esteem it a favour if you will allow this letter to appear in your journal. Enclosing my card, I subscribe myself,

October 17. PREVENTION.

INVITATION TO OBSERVE THE LUMINOUS NIGHT CLOUDS.[1]

SINCE the year 1885 a very remarkable phenomenon has been noticed in the sky in our latitudes, which well deserves to excite the interest of astronomers and geophysicists. The following is the substance of what has so far become known regarding the so-called luminous night-clouds.

In the latitude of Berlin the phenomenon shows itself only during a comparatively short period of the year—from May 23 to August 11. While in the first years it was seen pretty frequently even before midnight, it has, during the last four years, rarely appeared except after midnight. The phenomenon appears in the form of cirrus-clouds, which come out bright on the twilight sky. This especially distinguishes them from the ordinary cirrus-clouds, which, with the depths of the sun in which the luminous clouds are seen at present, come out dark on the light twilight sky. The colour of the phenomenon is generally a bluish white, which becomes yellowish and reddish in close proximity to the horizon.

Often repeated photographs which have been taken simultaneously at various points in the neighbourhood of Berlin, show that the altitude of the luminous clouds is constant and exceedingly great—82 kilometres. In consequence of this great altitude, they receive light from the sun standing *below* the horizon, which makes them appear light on the twilight-sky. They are visible only so long as the sun shines on them; as soon as the shadow of the earth passes over them they become invisible. As a rule they begin in the morning, shortly before twilight, and they disappear as soon as the sun stands higher than 8° to 10° below the horizon.

Of late years these clouds have been seldom seen. Within the period above stated, they occurred this year only about ten times, while in the first years they were very frequent. Their appearance is subject to great changes; while they frequently exist only in a few little luminous stripes or patches, at times they appear in greater accumulations and with a more intense light. Especially in the last days of the period, from August 2 until 6, their light seems to be considerable in our latitudes. Generally they are observed in the proximity of the horizon—over that part of it under which the sun is.

Frequent observations of the movements of the phenomena, which, after midnight, are always from the direction of N.E. ± 40°, render it probable *that the movements are caused principally through the resisting medium of the mundane space*. In accordance with this is the fact that in the half-year after its appearance in this country, the phenomenon has been observed repeatedly in the southern latitudes of 53° by the meteorological observer, Mr. Stubenrauch, in Punta Arenas, as well as several times by ship-captains.

Other observations confirm the assumption of an annual wandering of this kind. For instance, in Graham's-town under 33° S. lat. the phenomenon was observed on October 27, 1890,[2] and in Haverford under 40° N. lat.; according to written information it was observed on May 17, 1892. These dates, taken in association with the time of the appearance in this country, directly indicate a wandering of the phenomenon from N. to S. and back.

The luminous night-clouds decrease year after year in respect to the frequency of their appearance as well as to their extent and to their intensity of light. The phenomenon therefore will entirely disappear within a few years. It seems, however, that during the next two years observations will still be possible, which may give us information regarding several questions of extraordinary importance.

Measurements of the apparent altitude of the upper limits of the luminous clouds, mainly in the time in which the upper limit of the twilight-segment has the comparatively small altitude of, say, 1° to 10°, would be of great value. Such measurements will serve to decide the question whether the altitude of the clouds varies under different geographical latitudes, providing that the measurements always refer to such points as lie within the upper limits of the clouds, produced by the shadow of the earth.

During the last few years the whole twilight-segment has been comparatively seldom filled out by the luminous night-clouds, and it may therefore frequently remain doubtful whether the highest point of the phenomenon really lies in the limit of the earth-shadow. In order to make sure that the measurements are adapted to their purpose, they must be repeated as often as possible in intervals of a few minutes. In the evening this limit is generally recognized by the fact that within it parts of the phenomenon disappear from above, while towards morning new parts always become visible within the limit upwards. The distance of the zenith of the upper limit of the luminous clouds in the vertical of the sun for the latitude of Berlin, presuming that the phenomenon stretches over the whole of the twilight-segment, may be seen from the following statement:—

Depth of the sun below the horizon.	Zenith distance of the uppermost limit.
°	°
12·0	80
12·5	83
13·0	85
13·5	86
14·0	87

Moreover, as by means of a telescope the upper limit of the phenomenon is generally seen a little higher than with the naked eye, it is desirable that the telescope should always be adjusted to the limit-line seen with the naked eye. A comparison of the appearance seen with the naked eye with the one seen through the telescope, will enable the observer to discover easily the line corresponding to the one seen with the naked eye. The exactitude of these measurements must be about 3′ to 6′, with respect to the azimuth and to the altitude, while the time should be exact within two to four seconds.

The employment of photographic apparatus is of advantage for the indication of the place, as well as of the movements, of the phenomenon. But only those kinds of apparatus are suitable in which the proportion of the diameter of the opening to the focal distance is at least 1 : 4 or greater. If the proportion is smaller, the duration of lighting will last too long, and consequently, on account of the quick changings of the phenomenon, the details will get lost. With an apparatus of which the proportion of the aperture-diameter to the focal distance is 1 : 3, the duration of lighting for the various depths of the sun below the horizon, on condition that the phenomenon is light in some degree, is as follows:—

Depth of the Sun below the Horizon.	Duration of Lighting.
°	s.
9	16
10	21
11	27
12	35
13	48
14	72
15	122

[1] Scientific journals are requested to reproduce this article.
[2] Compare *Astr. Nachr.*, No. 3008.

Generally at the same time stars become visible on the photographic plate, through which, in association with the time of photographing, the direction of adjustment of the apparatus is ascertained (that is to say, the position of the axle of the apparatus is ascertained).

With regard to equatorial regions, it is of great importance that the exact time in which luminous night-clouds pass through them should be determined. According to the observations hitherto made, the passing through the equator may take place in the time between the beginning of September and the end of October, and the return between the beginning of March and the end of April. Under 20° S. lat., the time of passing through will, in that case, be from the middle of September until the middle of November, and from the middle of February until the middle of April, and under 20° N. lat. from about the middle of March until the middle of M ay, and from the middle of August until the middle of October. In consequence of the daily rotation of the earth round its axis together with the distinct movements of the earth, atmosphere, it may be that the passing through the equator does not take place in the simple manner here described. It does not seem to be unlikely that the periods are not limited as exactly as stated.

Moreover, it is probable that the luminous night-clouds consist of a gas which is condensed in consequence of the lower temperature prevailing in the altitude of 82 kilometres. On the question relating to the nature of this gas depend several other cosmical questions: for instance, with respect to the temperature of the air of the mundane space and the temperature of the atmosphere at the altitude of 82 kilometres, which will be answered through comparing experiments in the laboratory. For this reason, spectrographs of the sunlight at low altitudes of the sun, in the season in which the phenomenon of the luminous night-clouds is seen, are of great value. Such spectrographs should be taken in the evening shortly before sunset, and in the morning shortly after sunrise.

It appears that in the northern regions of the earth, in about 70° latitude, in the period from the middle of June until the middle of July, an especially great accumulation of clouds takes place, which, however, on account of the sun standing constantly *above* the horizon during this period, will be hardly visible. It will, therefore, be of special advantage for these regions to take spectrographs of the sunlight at low positions of the sun.

These short remarks regarding the importance of the phenomenon with reference to cosmical problems may serve to show that the observations necessary for the exploration of the subject are well within the sphere of astronomers and geophysicists. There can be no doubt that the observations necessary for the solving of these questions are far beyond the capacity of a single institution. Those who take interest in the furtherance of the questions we have indicated are therefore requested to assist through one or other of the kinds of observation above noted in the investigation of the luminous night-clouds.[1]

W. FOERSTER.
O. JESSE.

Berlin Royal Observatory, September 1892.

SOME OPTICAL ILLUSIONS.[2]

A STRIKING illusion, first described by Zöllner some thirty years ago, and usually called by his name, appears in Fig. 1. Of the four main lines each adjoining pair seems to converge at one end, and to diverge at the other, whereas in reality the lines are all parallel. The first step in an explanation of the illusion would be the determination of its essential factors, of its various forms, and of some general principle embracing under one formula its several varieties. The next step would be to correlate this formulation with some recognized psychological principle. The generalization is found

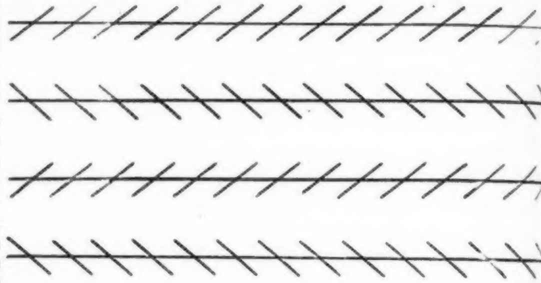

FIG. 1.

in the statement, that *the direction of the sides of an angle are deviated toward the direction of the angle*, and may be illustrated by reference to Fig. 2. In this figure the continuation of the left horizontal line seems to fall below the right horizontal line, and the continuation of the latter above the former; in reality the two are continuous. Similarly, if the continuations of the oblique lines be added, they will not seem continuous, but divergent slightly to one side or the other. If now we call the direction of an angle the direction of the line that bisects it, then the deviation is what would result from a drawing up of the sides of the angle towards this central bisecting line; the left end of the left horizontal line would be drawn up, and the right end of the right horizontal line would be drawn down, and thus the two seem discontinuous. The same would happen, though to a less

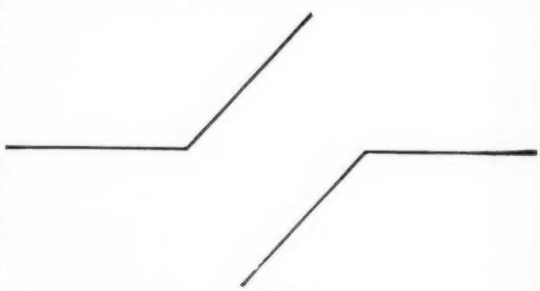

FIG. 2.

FIG. 3.

degree, if either oblique line were omitted. There are many other ways of illustrating this fact. Instead of drawing the right line horizontal, we may incline its right end downwards slightly, and then it will seem continuous with the left horizontal line. We may apparently incline both lines so that they would converge towards a point between and below them, as in Fig. 3 and the like. Two further points or corollaries should be noted: (1) that the

[1] A publication, "Die leuchtenden Nachtwolken," by O. Jesse, which may be expected within the next months, will contain details regarding the entire present position of these questions.

[2] Abstract of a paper on "A Study of Zöllner's Figures and other Related Illusions," by Joseph Jastrow, Ph.D. (with the assistance of Helen West), being a part of "Studies from the Laboratory of Experimental Psychology of the University of Wisconsin."—*American Journal of Psychology*, vol. iv. No. 3.

larger the angle the greater the deviation. Similar figures with acute angles substituted for the obtuse ones would show a scarcely perceptible illusion. (2) When obtuse

FIG. 4.

angles are combined with acute angles, the deviating effects of the former outweigh those of the latter. In Fig. 4 the effect of the angle ACD would be to make the

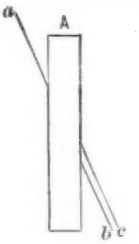

FIG. 5.

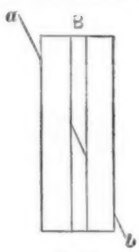

FIG. 6.

line AB if continued fall *below* FG, while the effect of BCD would be to make AB fall *above* FG; the former outweighs the latter, and the illusion appears as directed

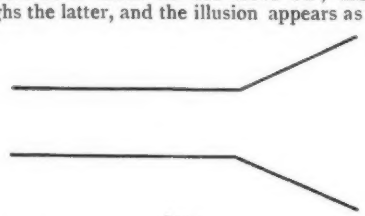

FIG. 7.

by the angle ACD. The angle BCE reinforces ACD, while ACE reinforces BCD. Angles greater than 180° do not come into consideration. When all the angles about a

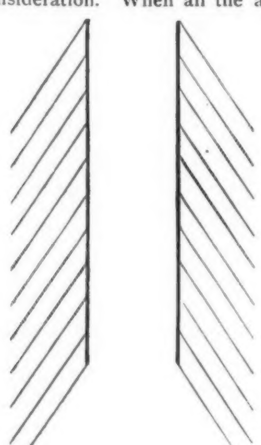

FIG. 8.

point are equal, *i.e.*, are right angles, the illusion disappears. Figs. 5 and 6 furnish other illustrations of the same principles. In Fig. 5 the line *a* seems continuous with *c* while it is so with *b*, and this because the obtuse angles formed by lines *a* and *c* with the vertical lines respectively, deviate the lines *a* and *c* towards the direction of the angles sufficiently to bring them in line with one another. Fig. 6 adds the further complication—explicable upon the same principles—that the line is deviated once in one direction and then in the reverse direction.

We have next to show that the illusion of deviation from

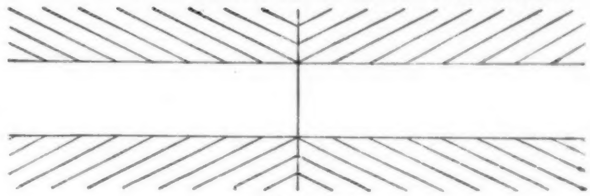

FIG. 9.

parallelism is similar to that from continuity. If the right-hand portion of Fig. 3 be rotated through 180° and placed below the left-hand portion, we have Fig. 7, in which we observe a tendency for the two horizontal lines to diverge on the left and converge on the right; this is just what our dictum demands. To strengthen this illusion we add more oblique lines, and thus more angles, the obtuse

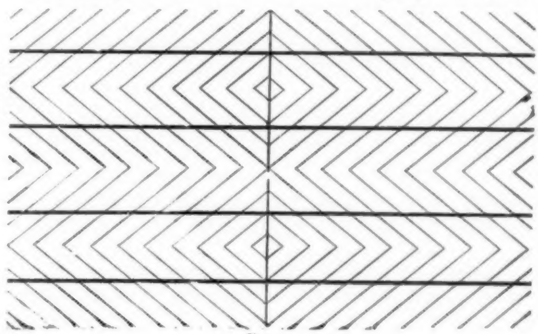

FIG. 10.

angles in all cases outweighing the acute ones—Fig. 8. We have now only to draw two figures like Fig. 8, side by side, and draw the oblique lines across the vertical ones (thus keeping the figure compact) to obtain Fig. 1, with which we set out. The possibilities of illusion do not stop here; by drawing the oblique lines in one direction on

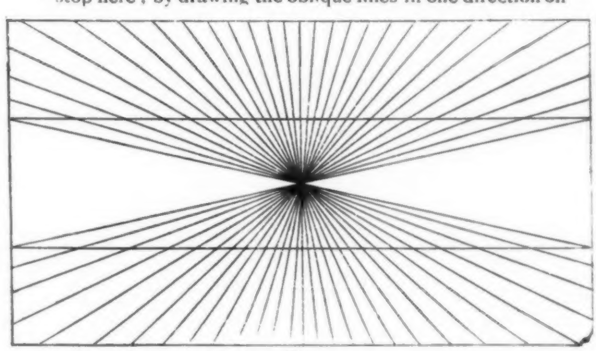

FIG. 11.

one side, and in the other direction on the other side, we can deviate the two halves of the same pair of parallel lines in opposite directions, as is done in Fig. 9; while most striking of all is the elaborate design of Fig. 10, in which it is difficult to realize that the four main lines are all straight and parallel. If the page be viewed with one

eye, and held horizontal nearly on a level with the eye, the true relations will appear. Fig. 11 is valuable for its conclusive demonstration that the deviation is proportional to the angle; the increasing angles gradually bend the straight lines away from one another, and give them the gradual change of direction of curves. These and other forms of illusion are all included in the generalization that the sides of an angle are deviated towards the direction of that angle.[1]

The psychological principle with which this general-

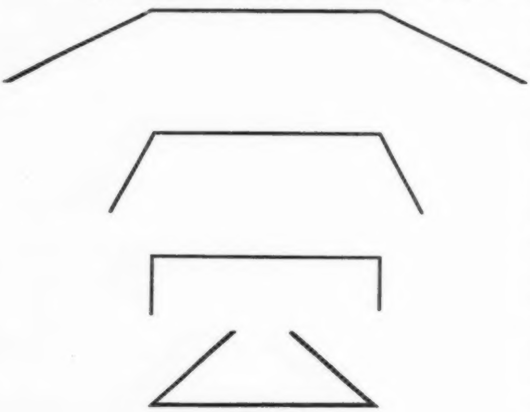

Fig. 12.

ization may be correlated is the law of relativity. This law emphasizes the fact that a sense-impression is not the same when presented alone and when in connection with other related sense-impressions. We cannot judge the direction of lines independently of that of the angles whose sides they form. As a further illustration of this principle it may be shown that angles will affect the apparent *lengths* of lines as well as their apparent *directions*. If in Fig. 12 we compare the horizontal portion of the uppermost figure with that of the lowest, it is

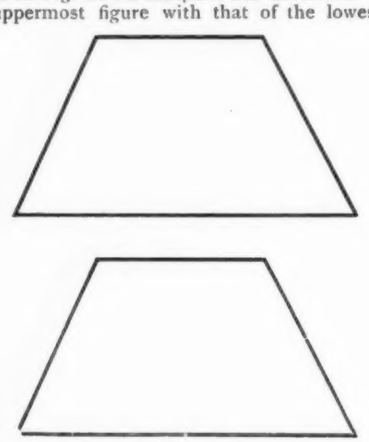

Fig. 13.

almost impossible to believe that they are of equal length. The intermediate horizontal lines seem intermediate in length, and thus illustrate the fact that the apparent length of the horizontal lines is directly proportional to the size of the angles at their extremities. The illusion would persist if we converted these figures into truncated pyramids by adding a line parallel to the horizontal line, and would then illustrate the fact that equal lines may be made to appear unequal by the effect of the areas whose contours they help to form. A converse effect is illustrated in Fig. 13 Here the upper figure seems larger than the lower, because its larger parallel side is brought into juxtaposition with the smaller parallel side of the lower figure. This illusion and others show especially well when cut out of paper and held against suitable backgrounds. As the figures are moved about one another the upper constantly becomes the larger. More than two figures may be used, and a variety of such contrasts may be formed.

The subject is by no means fully considered in these illustrations nor is the explanation offered as final or adequate. If it seems to direct investigation into fruitful paths its chief purpose will be accomplished.

[1] The reader is referred to the original paper for further illustration of this dictum, as well as for explanations of apparent exceptions and a discussion of the conditions affecting it.

THE NEW SATELLITE OF JUPITER.

THE new number of the *Astronomical Journal* contains Mr. Barnard's account of his discovery of this additional member *of our system*. We make the following extracts:—"Nothing of special importance was encountered until the night of September 9, when, in carefully examining the immediate region of the planet Jupiter, I detected an exceedingly small star close to the planet, and near the third satellite. I at once suspected this to be a new satellite. I at once measured the distance and position-angle of the object with reference to satellite three. I then tried to get measures referred to Jupiter, but found that one of the wires had got broken out and the other loosened. Before anything further could be done the object disappeared in the glare about Jupiter. Though I was positive the object was a new satellite, I had only the one set of measures, which was hardly proof enough for announcement. I replaced the wires the next morning. The next night with the great telescope being Prof. Schaeberle's, he very kindly gave the instrument up to me, and I had the pleasure of verifying the discovery, and secured a good set of measures at elongation. In these observations, and those of the succeeding night, only distances from the following limb of Jupiter could be measured. These were observed with the wires set perpendicular to the belts. The planet was thrown outside the field, the satellite bisected, and then the limb brought in and bisected also. This method would not permit any measures from the poles of the planet for latitude. On the 12th I inserted a strip of mica, carefully smoked, in front of the field-lens, for occulting the planet. This served admirably, permitting the satellite and planet to be both seen at once, and measures from the polar limbs could be made with great ease. The observations of the satellite from the 12th were all thus made.

"To avoid any personal equation I have on each night measured the diameters of the planet, for use in reducing the observations to the centre of Jupiter. Since the 12th, these have been measured through the smoked mica, so as to avoid introducing any error from the reduced brightness of the planet. The diameters were measured by the method of double distances. Just what the magnitude of the satellite is, it is at present quite impossible to tell. Taking into consideration its position, however, in the glare of Jupiter, it would, perhaps, not be fainter than the thirteenth magnitude. It will only be possible to settle this question with any certainty by waiting until some small star of the same magnitude is seen close to Jupiter, and then after determining its magnitude when away from the planet. In general the satellite has been faint—much more difficult than the satellites of Mars. On the 13th inst., however, when the air was very clear, it was quite easy.

"It is scarcely probable that this satellite will be seen

with anything less than twenty-six inches, and only with that under first-class conditions. I give here the observations that I have so far obtained, and defer any suggestions as to a name until a later paper. It certainly should not disturb the present harmony existing in the Roman numerals already applied to the satellites.

"It is so wholly different from any of the other moons in physical aspect, that it ought, in a sense, to be considered independent of them, and simply be called, say, the fifth satellite, with a suitable mythological name.

"It will be seen that on three of the dates of observation the east elongation is well covered in the measures."

Plotting the observations at elongation, the following values of the distance were obtained:—

			From Jupiter's centre. Miles.
September 10 (apparent)	61"·04	log R = 7·08267	·112250
" 12 "	61"·55	" 7·08452	·112750
" 14 "	61"·60	" 7·08324	·112400

From these the following periods result, using the well-known formula:—

$$P = p\sqrt{\frac{m}{M}\frac{R^3}{r^3}}.$$

	h.	m.
September 10 period	11	47·6
" 12 "	11	52·3
" 14 "	11	49·0
Mean	11	49·63

The observations, all made in standard Pacific time (eight hours slow of Greenwich) are given at length in the *Journal*.

The latitude measures show that the satellite's orbit lies in the plane of Jupiter's equator, and Mr. Barnard holds that it is consequently a very old member of Jupiter's family, "since it would doubtless take ages for the orbit to be so adjusted."

W. L.

NOTES.

THE ordinary general meeting of the Institution of Mechanical Engineers will be held on Wednesday evening, October 26, and Thursday evening, October 27, at 25, Great George Street, Westminster. The chair will be taken at half-past seven p.m. on each evening by the President, Dr. William Anderson, F.R.S. The ballot lists for the election of new members, associates, and graduates having been previously opened by the Council, the names of those elected will be announced to the meeting. The nomination of officers for election at the next annual general meeting will take place. The following papers will be read and discussed, as far as time permits:—Second Report of the Research Committee on the value of the steam-jacket, by Mr. Henry Davey, Chairman (Wednesday); and experiments on the arrangement of the surface of a screw-propeller, by Mr. William George Walker, of Bristol (Thursday).

WE are asked to intimate that the late Prof. Adams has left a number of separate copies of certain of his mathematical and astronomical papers, and that Mrs. Adams will be happy to distribute them to scientific friends who make application for them by letter addressed to her at 4, Brookside, Cambridge.

THE Harveian oration was delivered on Tuesday afternoon by Dr. J. H. Bridges. He presented an able and most interesting sketch of the scientific influences amid which Harvey's work was done, and the relation of his great discovery to later research.

THE controversy as to vivisection is still going on in the *Times*. For the present, therefore, it may be enough for us to reproduce the letter which was signed by Sir Andrew Clark, Sir James Paget, Dr. Samuel Wilks, and Sir George Humphry, and printed in the *Times* on Saturday last. It is as follows:—"Having already expressed our views, personally or by letter, to the Church Congress, we decline to enter into any further public discussion on the question of so-called 'vivisection,' for the following reasons, the statement of which we make solely because we think it is due to your readers:—Firstly, after full consideration, we are satisfied that the scientific aspect of this question cannot receive adequate and just treatment in the columns of a newspaper. Secondly, because it is hardly possible for us to name any progress of importance in medicine, surgery, or midwifery which has not been due to, or promoted by, this method of inquiry."

PROF. VIRCHOW was invested, on Saturday last, with the insignia of office as Rector of the University of Berlin. He chose "Learning and Research" as the subject of his address. He acknowledged that study had contributed greatly to create a mutual basis of understanding and a common educational foundation for the peoples of Europe, strengthening at the same time the idea of consanguinity. That state of things, however, was, he thought, entirely changed, and he contended that the turning-point in the supremacy of the classical languages had been reached. "A grammatical education is not the means for progressive development demanded by our youth. Mathematics, philosophy, and the natural sciences give young minds so firm an intellectual preparation that they can easily make themselves at home in any department of learning."

PROF. BERG has succeeded the late Dr. Burmeister as director, of the National Museum in Buenos Ayres.

DR. G. V. LAGERHEIM, at present director of the Botanic Garden at Quito, Ecuador, has been appointed curator of the museum at Trömso, Norway.

MR. W. G. RIDEWOOD, B.Sc., of the Royal College of Science, South Kensington, Assistant to the Director of the Natural History Museum, has been appointed Lecturer in Biology to the St. Mary's Hospital Medical School.

THE October number of the *Kew Bulletin* opens with a section giving some interesting information as to Lao tea. Some time ago a singular method of using the leaves of what has since been proved to be the Assam tea plant of commerce (*Camellia theifera*) was brought before the Society of Arts by Mr. Ernest Mason Satow. Amongst the Laos, a people inhabiting a district of Siam, in the neighbourhood of Chiengimai, the tea leaves are not used for making an infusion as in other countries, but are prepared wholly for the purpose of chewing. The leaves are first steamed and then tied up in bundles and buried in the ground for a period of about fifteen days. Leaves thus prepared, called locally "mieng," are said to keep for two years or more. The habit of chewing "mieng" is almost universal among the Laos, and to men engaged in hard work, such as poling or rowing, it is said to be almost indispensable. The *Bulletin* prints a correspondence in which the result of an inquiry made by Kew in regard to the plant yielding "mieng" and the method of preparation is detailed.

THE other sections in the October number of the *Kew Bulletin* deal with Chinese silkworm gut; mangrove bark and extract; Burmese black rice; Mauritius tea; potato disease in Poona; British North Borneo; and Allouya tubers. There are also some miscellaneous notes.

WE learn from the *Journal of Botany* that Dr. H. Trimen, F.R.S., the Director of the Botanic Gardens at Paradeniya Ceylon, has received the sanction of the Government to proceed with the publication of the flora of that island. The work will be published in parts by Messrs. Dulau and Co., and will form two vols. octavo, together with a quarto atlas of 100 coloured plates,

drawn by the native Cingalese artists attached to the gardens. The first part is now in the press. The book is more especially designed for use in the colony, and will enter into more local detail than has been hitherto the practice in the Colonial floras published by the Government.

MR. G. HOGBEN delivered an excellent address lately before the Canterbury College Science Society, New Zealand, on earthquakes. In the course of his remarks he described the system which, for the last three years, has been in force in New Zealand for the observation of earthquake phenomena and the telegraphing of the results to a central station. This system has been adopted in Victoria, New South Wales, South Australia, and Tasmania, and will probably be shortly adopted in Queensland. The various colonies exchange reports with New Zealand, and it is proposed that the system shall be further extended, so that the colonies may be brought into communication with the islands of the Pacific and America and Japan.

A REUTER telegram despatched from Vienna on October 14 announced that reports had reached that city of the occurrence of violent earthquake shocks in Eastern Europe. The vibrations were strongest in Roumania, being felt at Bucharest, where they lasted 15 seconds, and at Galatz during 30 seconds. At Oltenizza the shock lasted fully 90 seconds, and did considerable damage in the town. A shock was felt at Sofia on October 14, at seven o'clock A.M., and also at Philippopolis, Varna, and Rustchuk. The seismic wave passed from south to north, the vibration lasting several seconds, and being accompanied by subterranean rumbling.

THE depression over the Bay of Biscay referred to in our last issue took a very unusual route, the track being almost circular, moving first in an easterly and north-easterly direction towards the north of France, and then recurving by the south-west of England back to the Bay of Biscay, when it again travelled to the eastward. The disturbance caused to the weather in this country was very great and the rains were very heavy, with serious floods, especially in Wales and the midland and northern counties. In Yorkshire it rained almost incessantly from Thursday to Saturday, a fall of 1¾ inch being measured in one day. The weather was still further disturbed by an area of low pressure lying over the north of Germany between Sunday and Monday, which caused disastrous gales and further heavy rains in the eastern part of the country. The temperature has been very low for the season, the daily maxima scarcely reaching 55° in any part of the kingdom, owing to the persistent northerly and north-easterly winds. Towards the close of the period temperature fell several degrees lower, with sharp day frosts in Ireland, under the influence of an anti-cyclone, which spread over the country from the Atlantic, while hail and sleet showers fell in places. The *Weekly Weather Report* of the 15th inst. showed that the temperature of the past week was everywhere below the mean, being as much as 4° in the south-west of England and 5° in the south of Ireland. The rainfall greatly exceeded the average over the north and east of England.

AN anemometer by M. Timchenco of a novel construction is described by Prof. Klossovsky, of the Odessa Observatory, by which both the wind direction and velocity are marked on a cylinder by one symbol. The recording apparatus is moved by clockwork and the indications are made by electrical contacts. The duration of the contact depends upon the velocity of the wind, a light wind producing a contact of longer duration than a strong one. The indications are by means of arrows printed on the paper covering the cylinder, which show the direction of the wind, and the number of arrows marked on a length of paper corresponding to one hour furnishes data for finding the velocity by an empirical scale determined by comparison with a Robinson's anemometer. The apparatus only requires to be adjusted twice a month, or in some instruments only once a month, and calls for no attention in the meantime. A battery cell is sufficient to produce the contact, for most of the work is done by means of weights.

THE *Annuaire* of the Municipal Observatory of Montsouris for the years 1892–93 contains, in addition to the usual tables showing *inter alia* the extremes of temperature at Paris since 1699 and the monthly rainfall values since 1690, much useful information with reference to the climate and the microscopic examination of the quality of the air. Although it does not fall within the province of the observatory to issue weather forecasts, applications for such information are sometimes received and answered, in the interest of agriculture. The opinion is expressed that by basing the calculations on the general methods adopted by Laplace in his memoir entitled "Probabilité des causes d'après les événements," it is not impossible to give a long forecast which may be at times of much use. Some interesting remarks are also made as to the possibility of foreseeing the character of the summer from the weather experienced in the early spring, based chiefly on the time of the appearance of the north-east winds, and the differences in their usual strength and physical qualities, in connection with the transparency of the air. The results of the analysis of the air show that the minimum amount of carbonic acid occurs between May and September, and that the amount at night is greater than during the day.

THE new University of Chicago has decided that its work shall go on all the year through, including the summer months. According to the New York *Nation*, the calendar year is divided into four quarters of twelve weeks each, beginning respectively on the first days of October, January, April, and July; and at the end of each quarter there is to be a recess of one week. Each quarter consists of two terms of six weeks. No student is to be held to an attendance of more than three quarters, or six terms, in each year, so that the normal academic year is no longer than at other colleges. Each student is to begin his academic year whenever he is ready, and to take his quarter's vacation whenever it suits his convenience. He may even take his two terms of vacation in different quarters.

AN investigation of the phenomena exhibited at the negative poles of vacuum tubes appears in vol. xl. of the *Sitzungsberichte* of the Prussian Academy. Professor E. Goldstein considers that the term "stratification" as applied to the light at the cathode is a misnomer, since two at least of the strata can be shown to pervade the entire region of luminosity. The light nearest the cathode is yellowish, and about 1 cm. thick. Next comes Crookes' "dark space," which in reality shines with a faint blue light. Then follows the third and most highly luminous layer, whose colour changes from a blue to a violet as the exhaustion is carried further. The first layer was shown to be a separate phenomenon on a previous occasion. The so-called second layer shows the peculiarity of rectilinear propagation. It is emitted from the electrode normally to its surface, or very slightly divergent, whereas that of the third layer spreads throughout the bulb and even passes round corners. The second layer is best shown by concave poles, which concentrate the light at the centre of curvature. If observed through a blue glass, which cuts off the third layer, it is seen to diverge from the focus and impinge upon the wall of the bulb. The phosphorescence observed in the glass where it is struck by the "radiant matter" is due to this part of the light only, and not to the third layer. It is this also which produces the well-known phenomena of shadows. A glass rod laid in its path casts a shadow through the blue space, which is, however, relieved by the purple luminosity of the third layer. The former is also the only light deflected by a second cathode. It is to be concluded that the light at the negative pole of a vacuum tube consists of three different species, each pervading the others, but having distinct and characteristic properties of its own.

IN the current number of the "Annals of Scottish Natural History," Mr. E. P. Knubley discusses the question whether legislative protection is required for wild birds' eggs. He suggests that the most practicable plan might be for Parliament to grant powers to the County Councils from time to time, and as necessity arose, to place certain portions of a district, such as mountains, commons, waste places, lakes and meres, or portions of cliffs or foreshores, under an Act for specified months in the year—say, from April 1 to June 30. What, however, is most urgently wanted, as Mr. Knubley says, is that landlords and occupiers shall, as far as possible, protect birds breeding on their property or "occupation."

MR. ERNEST ANDERSON recently read before the Field Naturalists' Club of Victoria an interesting paper on some Victorian Lepidoptera. He said that a great charm accompanied the rearing out of the Victorian species, because the results were very frequently of a most unlooked-for character. The Victorian forms followed the same rule as many plants and animals in having characteristics and habits purely Australian; and not only so, but they helped to bridge over the sharply-defined divisions known in Europe, and merged the various groups so imperceptibly into each other that it was hard to say where one ended and another began. Speaking of the processional caterpillars (*Teara melanosticta*), Mr. Anderson described how a female laid some ova in a small box and covered them very thickly with yellow down. Very shortly afterwards a thread-like structure was visible, which close examination revealed to be composed of newly-hatched caterpillars in Indian file, each having its head close up to the tail of its forerunner, and the whole line moving simultaneously with mathematical precision.

THE use of gas engines does not seem to be nearly so common in the United States as in Great Britain. According to the *Railroad and Engineering Journal*, they are generally regarded in America as of service for light work only, and it is with some surprise that our contemporary has noted the advertisement of an English firm, which keeps all sizes up to forty-horse-power in stock, and offers to furnish single engines of any size up to two-hundred-and-fifty-horse-power. This much exceeds the capacity of any gas engine built until very recently.

THE U.S. Department of Agriculture has published a valuable account, by Harvey W. Wiley, of experiments with sugar-beets in 1891. The experiments were divided into three classes; (1) culture of the sugar-beet conducted by farmers in different parts of the country; (2) culture of the sugar-beet conducted by the agricultural experiment station of Wisconsin and numerous farmers in Wisconsin, under the direction of the agricultural experiment station of that State, by authority of the Secretary of Agriculture; (3) experiments conducted at the beet-sugar experiment station of the Department at Schuyler, Nebraska.

WE learn from the *American Naturalist* for October that the vertebrate fossils collected by Prof. Marsh, to which we lately referred, are not likely, after all, to be soon exhibited in the National Museum at Washington. Our contemporary says: "One side of a small room is the only space at present occupied by the material in question, and it is safe to say that no other space has been yet provided. As the National Museum committed the error at its establishment of attempting an exhibit of modern human industries, as we pointed out at the time, the space for scientific exhibits is necessarily greatly curtailed. The necessities of this department require the erection of a new building, and until that is done it is safe to say that the vertebrate collections of the U.S. Geological Survey will not be exhibited."

SLEEP is one of the least understood of physiological phenomena. A new theory of it (we learn from the *Revue Scientifique*) has been offered by Herr Rosenbaum. He supposes the essential fact in the fatigue of the nervous system leading to sleep to be a hydratation of the nerve-cells, an increase of their water-content. The greater the hydratation, the less the irritability. This hydratation arises through chemical change of the nervous substance during activity. A small part of the water escapes by day through the lungs, but the greater part is eliminated during sleep. Its passage into the blood takes place by virtue of the laws of diffusion, and depends on the quantity and density of the blood, its amount of fixed principles, its speed of flow, &c. Elimination of the expired air takes place according to the laws of diffusion of gases. The assimilable substances of the body-take the place of the water eliminated in sleep. The repair of the physical and mental forces through sleep is due to this elimination and replacement. Intelligence is in inverse ratio of the proportion of water in the brain, and may be measured by this proportion, at least in the child. It may be doubted whether this theory explains the sleep of hibernating animals, or that caused by opium and anæsthetics.

D. J. MADISON TAYLOR has been elaborately investigating the various problems relating to physical exercise in health and as a remedy, and some of the results are set forth in the *Journal of the Franklin Institute* for September and October. One conclusion is, he says, uniformly prominent in the instances of damage from boat and other racing. Always the training has been "either insufficient or bad, or both."

IN one of the papers contributed to the third number of the Trinidad Field Club's Journal, Mr. J. Edward Tanner describes some interesting observations of the habits of the Parasol or Leaf Cutting Ants. Two nests of these ants were on his table at the time when his paper was being prepared. He begins by noting that all in Trinidad who are interested in such subjects know the hurried manner in which a parasol ant returns to her nest (all leaf-cutting workers are females), bearing erect in her mandibles the portion of leaf she has herself just cut off, and apparently running home with it in triumph. These foragers, for they are the ones who supply the household, carry their portion of a leaf well into the nest, drop it, and return for another piece, nor do they cease doing so till the supply is more than those in the nest require. Mr. Tanner could not induce the ants in one of his nests to carry any leaf whatsoever into the nest, till one day he coaxed a small worker to do so. As she entered she was caressed by those in the nest, who stroked and patted her with their antennæ. The small piece of leaf she had brought was at once taken by one of the larger workers, to go through its various processes, while she returned for more, and she continued to bring in pieces till late in the evening. Strange to say, none of the others followed her example. Even four weeks later only two or three carried in any portions of leaf. Mr. Tanner suggests that this may have been due to the fact that the queen was accidentally killed while the nest was being taken. The other nest had a queen, and with it there was no trouble, for the ants kept themselves well supplied from whatever was offered to them on their feeding ground, whether rose leaves, plumbago, or quis-qualis. "Each forager," says Mr. Tanner, "drops her portion of leaf in the nest, which is taken up as required by the small workers, and carried to a clear space in the nest to be cleaned. This is done with their mandibles, and if considered too large it is cut into smaller pieces. It is then taken in hand by the larger workers, who lick it with their tongues. Then comes the most important part, which almost always is done by the larger workers, who manipulate it between their mandibles, mostly standing on three legs. The portion of leaf is turned round and round between the mandibles, the ant using her palpi, tongue, her three legs, and her antennæ while doing so. It

now becomes a small, almost black ball, varying in size from a mustard seed to the finest dust shot, according to the size of the piece of leaf that had been manipulated. The size of the piece of leaf is from an $\frac{1}{8}$ by $\frac{1}{8}$ of an inch, by $\frac{1}{4}$ by $\frac{1}{4}$ of an inch. I do not wish it to be understood that only one class of workers manipulate the leaf, for all seem to take to it very kindly on emergency. Even the smallest workers will bring their tiny ball to where the fungus bed is being prepared. These balls, really pulp, are built on to an edge of the fungus bed by the larger workers, and are slightly smoothed down as the work proceeds. The new surface is then planted by the smaller workers, by slips of the fungus brought from the older parts of the nest. Each plant is planted separately and they know exactly how far apart the plants should be. It sometimes looks as if the plants had been put in too scantily in places, yet in about forty hours, if the humidity has been properly regulated, it is all evenly covered with a mantle, as if of very fine snow. It is this fungus they eat, and with small portions of it the workers feed the larvæ."

MR. O. P. HAY records in the latest volume of the Proceedings of the U.S. National Museum a curious habit of horned toads. Some years ago two boys from Texas, whose family had moved into his neighbourhood, showed him a few lizards belonging to the genus Phrynosoma, and popularly called horned toads. The boys declared that these little animals, when teased, would sometimes squirt blood out of their eyes. Mr. Hay did not think much about the matter at the time, but was lately vividly reminded of it in the department of reptiles in the National Museum. Near his desk there was a specimen of *Phrynosoma coronatum*, which had been sent from California by a member of Dr. Merriam's exploring party. About August 1 it was shedding its outer skin, and the process appeared to be a difficult one, since the skin was dried and adhered closely. One day it occurred to Mr. Hay that it might facilitate matters if he gave the animal a wetting; so, taking it up, he carried it to a wash-basin of water near by and suddenly tossed the lizard into the water. "The first surprise," says Mr. Hay, "was probably experienced by the Phrynosoma, but the next surprise was my own, for on one side of the basin there suddenly appeared a number of spots of red fluid, which resembled blood." He immediately recalled what the boys had told him of the ability of horned toads to squirt blood, and he concluded that this was a good time to settle the question whether this fluid was blood or not. A microscope was soon procured and an examination was made, which immediately showed that the matter ejected was really blood. A day or two afterwards Mr. Hay was holding the lizard between his thumb and middle finger, and stroking its horns with his forefinger. All at once a quantity of blood was thrown out against his fingers, and a portion of it ran down on the animal's neck; and this blood came directly out of the right eye. Mr. Hay has since found that the phenomenon has been noticed by other observers, and, while he was preparing his paper, his attention was called to the fact that more than twenty years ago Mr. A. R. Wallace read before the Zoological Society of London letters from a correspondent in California, who described this creature as squirting from one of its eyes "a jet of bright red liquid very much like blood."

MESSRS. PERCIVAL AND CO. announce the following works:—"Geometrical Drawing," by A. J. Pressland; "Lessons on Air," by A. E. Hawkins; "The School Euclid," an edition of Euclid, Books i.–vi., with Notes and Exercises, by Daniel Brent; and a series of elementary text-books entitled "The Beginner's Text-books of Science," of which Mr. G. Stallard is the general editor.

MESSRS. GEORGE BELL AND SONS have published a second edition of Mr. A. J. Jukes-Browne's "Student's Handbook of Physical Geology." The author explains that in preparing this edition he has spared no pains to make it a trustworthy handbook for those branches of the science to which it relates.

MESSRS. WHITTAKER AND CO. have issued for the benefit of amateur coil-makers a practical manual on "Induction Coils," by G. E. Bonney. The author's object has been to place in the hands of his readers "a cheap and handy volume giving a general insight into the construction of ordinary spark coils, medical coils, and batteries for working them." There are more than a hundred illustrations.

MESSRS. LONGMANS, GREEN, AND CO. have issued a new edition, revised and largely re-written, of the well-known "Outlines of Psychology," by Prof. James Sully.

MESSRS. CHAPMAN AND HALL will shortly publish a work by Rev. H. N. Hutchinson, entitled "Extinct Monsters." It will be illustrated by Mr. J. Smit, who has made twenty-four restorations of antediluvian animals. The book is not intended for geologists only, but for all who are interested in the study of animal life. Dr. Henry Woodward, F.R.S., keeper of geology, Natural History Museum, contributes a preface.

THE new number of *Records of the Australian Museum* (Vol. III., No. 2) opens with a paper, by Mr. J. Douglas Ogilby, on some undescribed reptiles and fishes from Australia. To the same number Mr. C. Hedley contributes a paper on the structure and affinities of *Panda Atomata*, Gray. Mr. A. North has a note on *Manucodia comrii*, Sclater.

THE University College of North Wales has published its calendar for the year 1892-93.

GLYCOL aldehyde, $CH_2OH.CHO$, the hitherto almost unknown first aldehyde derived from glycol, forms the subject of a communication to the current number of the *Berichte* by Prof. Emil Fischer and Dr. Landsteiner. This substance acquires additional interest when the ordinary sugars are defined as aldehyde- or ketone-alcohols, for it then becomes the first member of the series. Prof. Fischer now shows how glycol aldehyde may readily be obtained, discusses its properties, and points out that by its polymerisation a new sugar is obtained, tetrose, the first synthetical sugar containing four atoms of carbon. The only evidence hitherto published of the existence of glycol aldehyde is that afforded by the work of Abeljanz and Pinner. The former chemist considered that he had obtained it by heating di-chlor-ether with water, and by the action of sulphuric acid upon mono-chlor-hydroxy-ether. But upon repeating the work of Abeljanz, Prof. Fischer finds that the substance considered, upon very slight evidence, to be glycol aldehyde is another compound altogether. Pinner afterwards attempted to obtain it by decomposition of a substance discovered by him, glycol acetal, with acids, but Prof. Fischer finds that this reaction only occurs under conditions such that the glycol aldehyde is itself also decomposed. In view of the formation of glyceryl aldehyde by the action of baryta upon acroleïn dibromide, a reaction now of historical importance as being the one which led Prof. Fischer to the first synthesis of grape sugar, it was thought probable that glycol aldehyde might be similarly obtained by the action of baryta upon the mono-bromine derivative of aldehyde, $CH_2Br.CHO$. Mono-brom-aldehyde, however, had never been hitherto obtained, so Prof. Fischer and Dr. Landsteiner first sought a method for its preparation. They eventually obtained it, as a viscid colourless liquid of powerful tear-exciting odour, by heating mono-brom-acetal, $CH_2Br.CH\begin{smallmatrix} \diagup OC_2H_5 \\ \diagdown OC_2H_5 \end{smallmatrix}$, with anhydrous oxalic acid. When the mono-brom-aldehyde thus

obtained was mixed with water containing barium hydrate partly in solution and partly in suspension, and the whole maintained for half an hour at 0°, the odour of the brom-aldehyde disappeared almost completely. Upon removal of the baryta by sulphuric acid and the hydrobromic and sulphuric acids by lead carbonate, the filtered liquid was found to contain glycol aldehyde, which could be concentrated by evaporation over oil of vitriol *in vacuo*. The solution of glycol aldehyde reduces Fehling's solution with great energy at the ordinary temperature. When warmed with a solution of phenylhydrazine in acetic acid crystals of an osazone are deposited, just as happens in the case of other members of the series of sugars. Glycol aldehyde is readily oxidized by bromine water to glycollic acid, $CH_2OH.COOH$. When treated with a dilute solution of sodium hydrate polymerization occurs, a sugar of the composition $C_4H_8O_4$, the first synthetical tetrose, being formed, which is readily isolated in the form of its osazone (phenyl. hydrazine compound). This osazone appears to be identical with one obtained by Prof. Fischer from one of the oxidation products of natural erythrite. The preparation of glycol aldehyde completes the synthesis of the whole of the members of the series of sugars, from the first member up to the sugars containing nine atoms of carbon, with the exception of pentose. This latter sugar Prof. Fischer hopes shortly to obtain from the tetrose above described.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseo-viridis* ♀) from Zanzibar, a Bengal Fox (*Canis bengalensis*) from Pondicherry, presented by the Rev. J. W. Scarlett; a —— Monkey (*Cercopithecus sp. inc.*) from the Zambesi, presented by Mr. Joseph A. Moloney; a Bonnet Monkey (*Macacus sinicus*) from India, a White Stork (*Ciconia alba*), European, presented by the Rev. Sidney Vatcher; a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by Miss Synge; a Hairy Armadillo (*Dasypus villosus*) from South America, presented by Mr. J. H. Hamilton Benn; a Common Badger (*Meles taxus*), British, presented by Mr. W. Butler; a —— Galago (*Galago sp. inc.*) from East Africa, presented by Mr. Thomas E. C. Remington,; an —— Ichneumon (——), a Purple-crested Touracou (*Corythaix porphyreolophus*), two Black Gallinules (*Limnocorax niger*), a Tambourine Pigeon (*Tympanistria bicolor*), an Emerald Dove (*Chalcopelia afer*), four Half-collared Doves (*Turtur semitorquatus*), a —— Fruit Pigeon (*Treron sp. inc.*), four —— Tree Frogs (*Hylambates maculatus*), seven Smooth-clawed Frogs (*Xenopus lævis*) from East Africa, presented by General Mathews; three Mitred Guinea Fowls (*Numida mitrata*), a —— Snake (*Philothamnus semivariegatus*) from East Africa, presented by Mr. W. Hall Buxton MacDonald, M.D.; a —— Pratincole (*Glareola sp. inc.*), a Half-collared Dove (*Turtur semitorquatus*), a Nilotic Crocodile (*Crocodilus niloticus*) from East Africa, presented by Mr. R. MacAllister; two —— Francolins (*Francolinus* ——), a —— Coucal (*Centropus* ——), five Half-collared Doves (*Turtur semitorquatus*) from East Africa, a Black-tailed Hawfinch *Coccothraustes melanurus*) from Japan, presented by Mr. F. Pordage; a Flap-necked Chameleon (*Chamæleon dilepis*), two Square-marked Toads (*Bufo regularis*) from East Africa, presented by Mr. E. Millar; a Galeated Pentonyx (*Pelomedusa galeata*), two —— Skinks (*Gerrhosaurus major*), five —— Geckos (*Hemidactylus mabouia*), three —— Lizards (*Mabuia striata*) from East Africa, presented by Mr. Frank Finn, F.Z.S.; a Common Quail (*Coturnix communis*), captured at sea, presented by Mr. A. Torrie; a Honey Buzzard (*Buteo apivorus*) from France, presented by M. S. A. Pichot C.M.Z.S.; a Burrowing Owl (*Speotyto cunicularia*) from South America, presented by Mr. R. B. Shipway; two Common Boas (*Boa constrictor*) from Trinidad, presented by Messrs. Mole and Urich; a Black-headed Lemur (*Lemur brunneus*) from Madagascar, a Yellow-tailed Rat Snake (*Spilotes corais*) from Trinidad, deposited; an African Wild Ass (*Equus tæniopus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

A NEW COMET.—A telegram from Kiel announces the discovery of a new comet by Prof. Barnard on October 12 last, at 17h. 12·2m. mean Lick time. The position, as therein stated, was R.A. 293° 29′, and Declination + 12° 33′. As this new comet is termed "very dim," as seen with the large Lick refractor, it is needless to say that few instruments can at present observe it.

OUR SUN'S HISTORY.—The question of "How our Sun commenced to grow hot," is the subject of an article by Lord Kelvin in the October number of *L'Astronomie*. In these few pages he deals with various questions, among which may be mentioned: What is the temperature of the Sun? Is it increasing or diminishing? What was the state of the matter constituting our Sun before it was united into a single mass and began to grow hot? The answer to the last question leads him into the method of construction of our solar system. In considering the question of the encounter between two bodies as the origin, he finds that the probability of such an encounter between two neighbouring stars belonging to a large number of bodies, attracting one another mutually, and scattered in space, is much greater if they are at rest than if they are moving, even if their velocities are greater than those acquired in falling from rest. As an explanation of this Lord Kelvin takes the case of two solid and cold bodies of diameters equal to half that of the sun, and of mean densities equal to that of the earth, and supposes them at rest, the mean distance between each other being that of the earth from the sun. The collision caused by mutual attraction will transform the bodies into a fluid, incandescent mass, and he describes how this mass will arrange itself round this surface of collision. The next case he takes is similar to the one above, only the bodies have originally considerable velocities. Further on, as a special instance, he assumes the presence of 29 millions of solid cold globes, each having a mass equal to that of our moon, and the total masses of which are equivalent to that of our sun. These bodies, absolutely at rest, are supposed to be disseminated uniformly on the surface of a sphere (radius = terrestrial orbit), and allowed to fall towards the centre of the sphere by attraction. The result, to state briefly, is a mass of highly heated vapour, which afterwards expands and contracts consecutively, forming a gaseous nebula, measuring forty times the radius of the terrestrial orbit. By supposing that, instead of absolute rest at the commencement, these moons have a certain movement, the total sum of which represents a moment of rotation round a certain axis, equal to the moment of rotation of the solar system, this nebula would be a more or less facsimile of our solar system in its earlier stage, as figured out by La Place for his nebular theory. Thus this theory, "founded by La Place on the history of the sidereal universe such as Herschel observed, and completed in its details by his profound dynamical judgment and imaginative genius, appears to-day a truth demonstrated by thermodynamics." For the theory of the sun, Lord Kelvin says in conclusion that the antecedents immediately before incandescence cannot definitely stated, since the latter may have been caused by large and few bodies, or by agglomerations of such bodies as meteorites.

SILVERING GLASS MIRRORS.—Mr. Common, in the *Observatory* for October, gives a brief account of various processes and methods for producing good reflecting surfaces. In the short historical sketch we find that the modern process is due to an observation of Baron Liebig, who, in 1835, found that on heating aldehyde with an ammoniacal solution of silver in a glass vessel a brilliant deposit of metallic silver was deposited on the surface of the glass. In all the methods used up till quite recently the surface to be silvered had to be suspended over the bath, owing to the formation of mud which settles down and prevents the proper deposition of silver; thus really large surfaces could not be dealt with. This was the case with Mr. Common's 3-foot, a pneumatic arrangement being made to hold the mirror by the back. In dealing with the 5-foot, this method could not be so easily applied, and experiments were made to find some means by which this "mud" could be entirely eliminated. This was successfully

done by omitting the potash from the bath. One curious fact of observation is that the mirrors experimented on never seemed to take the first application of the silvering solution, but on being recleaned with nitric acid the second was always successful. Why this should be so does not seem to be easily explained, for Mr. Common only commits himself to the statement that "the nature of the liquid other than distilled water last in contact with the surface of the mirror seems to be the determining thing."

Himmel und Erde.—In this magazine for October there is a most interesting set of articles, of which we mention the following:—"Meteorology as the Physics of the Atmosphere," by Herr Wilhelm von Bezold. This comprises a general summary of the proceedings of the German Meteorological Society, which met in Braunschweig on June 7 last.—"Astronomy of the Invisible," by Herr Dr. Scheiner. This is the first of a series of articles, and deals, as far as it goes, with the discovery of Neptune by Adams and Le Verrier; it contains also a translation of the letter which Le Verrier wrote to Dr. Galle, who was then an assistant at the Berlin Observatory, telling him the results he had obtained, and asking him to make a search for the unknown planet. As a matter of interest we will give the elements of Neptune as obtained by Le Verrier and Adams, together with the true ones afterwards determined, for the results of such a piece of work will always be looked upon with interest.

	Le Verrier.	Adams.	True elements.
Half major axis ...	... 36·15	... 37·25	... 30·05
Eccentricity ...	... 0·1076	... 0·1206	... 0·0090
Longitude of Perihelion ...	285°	... 299°	... 46°
Mass (Sun = 1) ...	... 0·0001	... 0·00015	... 0·00005
Inclination	... 0°	... 0°	... 1°·8

In the notes two excellent illustrations of parts of the moon are inserted, one being a reproduction of a photograph taken at the Lick Observatory on August 31, 1890, and the other displaying the region to the north of Hyginus, showing these curious river-like appearances as first remarked by Prof. Weinek of Prague. Other notes deal with the astronomical reasons of the Ice Age, observations of Mars during the period 1883–88, polariscope observation of the surface of Venus, &c.

GEOGRAPHICAL NOTES.

MOUNT ORIZABA, or Citlaltepetl, in Mexico, has been measured trigonometrically by Mr. J. T. Scovell, with the result that its height is fixed as 18,314 feet. Popocatepetl is about 700 feet lower, and unless Mount St. Elias is found to considerably exceed Russel's estimate of 18,100 feet, Orizaba must be considered the highest summit in North America.

THE pumping of brine from the North German salt mines and the consequent subsidence of the land, is the cause of a somewhat interesting change in the small lakes near Mansfeld. The Salzigen See, as observed by Dr. Ule, of Halle, had a maximum depth of thirty metres on June 18, and of no less than forty-two metres on June 28, the subsidence of the bottom having taken place at the average rate of more than one metre a day.

FOLLOWING the death of Dr. Theodor Menke (see p. 302) we have to notice the loss of his fellow-worker, Dr. Karl Spruner von Merz, at the age of eighty-nine. He died on August 24, 1892. After a military career of some distinction, he retired from the army in 1886. His attention was early attracted to historical geography, and his famous "Historical Atlas" (1837–1852) has made his memory imperishable. It was in preparing the third edition of this atlas that he was first associated with Menke.

THE camels which were introduced into German South-West Africa last year, have, according to the *Deutsches Kolonialblatt*, proved a great success. They are employed in keeping up communication between Walfisch Bay and Windhoek, and for journeys into the interior. Their power of travelling for a week at a time without food or water has frequently been put to the test on the borders of the Kalahari desert. The climate does not seem to affect them unfavourably, and they have proved exempt from the many fatal diseases which attack horses and even oxen in Namaqualand.

A LECTURE on "Regions and Races" was delivered on Monday evening in the Regent's Square Hall by Dr. H. R. Mill. The object of the lecture was to demonstrate the continuity of geography with the physical sciences which account for the growth of the surface features of the globe, and with the natural sciences which explain the forms of plant and animal life on its surface. The interactions between man and his environment were discussed as the true basis of the higher geography.

M. J. THOULET has this summer been engaged in an oceanographical study of the Basin of Arcachon, and publishes in the last number of the *Comptes Rendus* an interesting epitome of his preliminary results. This lagoon forms a valuable oyster preserve, and the researches into the action of the tides on the enclosed water has practical as well as scientific bearings. The investigation will be continued, so as to include the other lagoons along the coast enclosed by sand dunes, and more or less cut off from the sea.

THE COMPARATIVE PHYSIOLOGY OF RESPIRATION.[1]

AMONG the very first of the physiological acts observed were those of respiration. The regular movements of breathing, from the first feeble efforts of the new-born babe until the sigh in the last breath of the dying—after which is silence, cold, and dissolution—have commanded the attention and claimed the interest of every one, the thoughtful and the thoughtless alike. And one comes to feel that in some mysterious way "the breath is the life." But in what way does breathing subserve life or render it possible? Aristotle and the naturalists of the olden time supposed that it was to cool the blood that the air was taken into the lungs, and, as they supposed, also into the arteries. With the limited knowledge of anatomy in those early days, and the fact that after death the arteries are wholly or almost wholly devoid of blood, while the veins are filled with it, what could be more natural than to suppose that the arteries were vessels for the cooling air? If one supposes that he has entirely outgrown this view of Aristotle, let him think for a moment how he would express the fact that an individual is descended from the Puritans, for example. In expressing it even the physiologist could hardly bring himself to say other than "he has the blood of the Puritans in his veins." Would he say "he has the blood of the Puritans in his arteries"?

As observation increased the cold-blooded animals were more carefully studied and found to possess also a respiration; they certainly do not need it to cool the blood. Then there are the insects and the other myriads of living forms that teem in the oceans, lakes, rivers, and even in the wayside pools. Do these, too, have a breath? And the plants on the land and in the water, is the air vital to them? Aristotle and the older naturalists could not answer these questions. To them, on the respiratory side at least, all life was not in any sense the same. It was not till chemistry and physics were considerably developed, not until the air-pump, the balance, and the burette were perfected that it was possible to give more than a tentative answer. It was not, until the microscope could increase the range of the eye into the fields of the infinitely little, possible to form even an approximately correct conception. The first glimmerings of the real significance of respiration for all living things was in the observation that the air which would not support a flame, although it might be breathed, could not support life. That is, there must be something in the transparent air that feeds the flame and becomes the breath of life, the real *pabulum vitæ*, the merely mechanical action of the air not being sufficient.

Since the experiments on insects and other animals by Boyle (1670) with the air-pump, by Bernouilli, on subjecting fishes to water out of which all the air had been boiled, and those of Mayow (1674), it became more and more evident that respiration was not confined to the higher forms, but was a universal fact in the organic world. Then came the most fruitful discoveries of all, made by the immortal Priestley (1775–6), viz., that the air is not an element, but composed of two constituents—nitrogen, which is inert in respiration, and oxygen, which is the real vital substance of the air, the substance which supports the flame of the burning candle and the life of the animal as well.

What would seem more simple at this stage of knowledge

[1] Address delivered, in August, 1892, at the meeting of the American Association for the Advancement of Science, by Prof. Simon Henry Gage, of Cornell University, Ithaca, N.Y., Vice-President of the Biological Section

than that the parallel between the burning candle and the living organism should be thought to represent truly the real conditions? That as the candle consumes the oxygen, burns, and gives out carbon dioxide, so the living thing breathes in oxygen, and gives out in place of that consumed carbon dioxide. And as in each case heat is produced, what would be more natural than to look upon respiration as a simple combustion? This was the generalization of Lavoisier (1780–89). As he saw it, the oxygen entered the lungs, reached the blood, and burned the carbonaceous waste there found, and was immediately given out in connection with the carbon with which it had united, and as the gas given off in a burning candle makes clear lime-water turbid, so the breath produces a like turbidity.

But here, as in many of the processes of nature, the end products or acts were alone apparent, and while the fundamental idea is probably true that respiration is, in its essential process, a kind of combustion or oxidation, yet the seat of this action is not the lungs or blood. If the myriads of microscopic forms are considered, these have no lungs, no blood, and many of them even no organs; they are, as has been well said, organless organisms, and yet every investigation since the time of Vinci and Von Helmont, Boyle and Mayow, has rendered it more and more certain that every living thing must in some way be supplied with the vital air or oxygen, and that this is in some way deteriorated by use; and the nearer investigation approaches to the real life-stuff or protoplasm, it alone is found to be the true breather, the true respirer, as was shown long ago by Spallanzani (1803–7). If one of the higher animals, as a frog, is decapitated and some of its muscle or other tissue exposed in a moist place, it will continue to take up oxygen and give out carbon dioxide, thus apparently showing that the tissues of the highly organized frog, may, under favourable conditions, absorb oxygen directly from the surrounding medium, and return to it directly the waste carbon dioxide. This shows conclusively that it is the living substance that breathes, and the elaborate machinery of lungs, heart, and blood-vessels, are only to make sure that the living matter, far removed from the external air, shall not be suffocated. Still more strange, it has been found that if some of the living tissue is placed in an atmosphere of hydrogen or nitrogen entirely devoid of oxygen, it will perform its vital functions for a while, and although no oxygen can be obtained, it will give off carbon dioxide as in the ordinary air. If it is asked, "how can these things be?" the answer is apparently plain and direct. Not as the oxygen unites directly with the carbon in the burning candle does it act in the living substance. The oxidations are not direct in living matter, as in the candle; but the living matter first takes the oxygen and makes it an integral part of itself, as it does the carbon and nitrogen and other elements; and, finally, when energy is to be liberated, the oxidation occurs, and the carbon dioxide appears as a waste product.

The oxygen that is breathed to-day, like the carbon or the nitrogen that is eaten, may be stored away and represent only so much potential energy to be used at some future time in mental or physical action.

So far only living animal substance has been discussed. If plants are considered, what can be said of their relations to the air? The answer was given in part by Priestley (1771), who found that air which had been vitiated by animal respiration became pure and respirable again by the action of green plants. He thus discovered the harmonizing and mutual action of animals and plants upon the atmosphere; and there is no more beautiful harmony in nature. Animals use the oxygen of the air and give to it carbon dioxide, which soon renders it unfit for respiration; but the green plants take the carbon dioxide, retain the carbon as food and return the oxygen to the air as a waste product. This is as thoroughly established as any fact in plant physiology; and yet, in his experiments, Priestley had some of what he called "bad experiments"; for instead of the plants giving out oxygen and thus purifying the air, they sometimes gave off carbon dioxide, and thus rendered it more impure, after the manner of an animal. What investigator cannot sympathize with Priestley when he calls these "bad experiments"; they appeared so rudely to put discord into his discovered harmony of Nature. But Nature is infinitely greater than man dreams. The "bad experiments" were among the most fruitful in the history of scientific discovery. Ingenhausz (1787) follcwed them up, carefully observing all the conditions, and found that it was only in daylight that green plants gave out oxygen; in darkness or insufficient light they conducted themselves like animals, taking up oxygen and giving out carbon dioxide. Finally it was proved by Saussure (1804) and others that for green plants, and those without green, like the mushroom, oxygen is as necessary for life as for animals. It thus became evident that this use of oxygen and excretion of carbon dioxide was a property of living matter, and that the very energy that set free the oxygen of the carbon dioxide was derived from oxidations in the green plant comparable with those giving rise to energy in animals. Further that the purification of the air by green plants in light is a separate function—a chlorophyll function, as it has been happily termed by Bernard—and resembles somewhat digestion in animals, the oxygen being discarded as a waste product. Indeed so powerful is the effort made to obtain oxygen for the life processes by some of the lowest plants—the so-called organized ferments—that some of the most useful and some of the most deleterious products are due to their respiratory activity. In alcoholic fermentation, as clearly pointed out by Pasteur and Bernard, the living ferment is removed from all sources of free oxygen, and in the effort for respiration the molecules of the sugar are decomposed or rearranged and a certain amount of oxygen set free.

It has been found that the motile power of some bacteria like *Bacterium termo* depends on the presence of free oxygen in the liquid containing them. When this is absent, they become quiescent. This fact has been utilized by Engelmann and others in the study of the evolution of oxygen by green and other coloured water plants. The bacteria serving as the most delicate imaginable oxygen test, so that when the minutest green plant is illuminated by sufficient daylight, the previously quiescent bacteria move with great vigour and surround it in swarms. Out of the range of the plant, the bacteria are still, or move very slowly, as if to conserve the minute energy-developing substance they have in store until it can be used to the best advantage.

May we not now approach the problem directly, and answer for the whole organic living world the question, "What is respiration?" by saying it is the taking up of oxygen and the giving out of carbon dioxide by living matter? This is the universal and essential fact with all living things, whether they are animals or plants, whether they live in the water or on land. But the ways by which this fundamental life process is made possible, the mechanisms employed to bring the oxygen in contact with the living matter, and to remove the carbon dioxide from it, are almost as varied as the groups of animals, each group seeming to have worked out the problem in accordance with its special needs. It is possible, however, in tracing out these complex and varied methods and mechanisms, to discover two great methods—the Direct and the Indirect.

In the first, there is the direct assumption of oxygen from the surrounding medium, and the excretion of carbon dioxide directly into it. The best examples of this are presented by unicellular forms like the amœba, where the living substance is small in amount, and everywhere laved by the respiratory medium. But as higher and higher forms are destined to appear, evidently the minute, organless amœba could not in itself realize the great aim toward which Nature was moving. There must be an aggregation of amœbas, some of them serving for one purpose and some for another. Like human society, as civilization advances, each individual does fewer things, becomes in some ways less independent, but in a narrow sphere acquires a marvellous proficiency. Or, to use the technical language of science, in order to advance there must be aggregation of mass, differentiation of structure, and specialization of function. Evidently, however, if there is an aggregation of mass, some of the mass is liable to be so far removed from the supply of oxygen, and the space into which carbon dioxide can be eliminated, that it is liable to be starved for the one and poisoned by the other. Nature adopted two simple ways to obviate this—first to form its aggregated masses in the form of a network or sponge, with intervening channels through which a constant stream of fresh water may be made to circulate, so that each individual cell of the mass could take its oxygen and eliminate its carbon dioxide with the same directness as its simple prototype, the amœba.

But in the course of evolution forms appeared with aerial respiration, and the insects, among these, solved the mechanical difficulty of respiration by a most marvellous system of air-tubes, or tracheæ, extending from the free surface, and therefore from the surrounding air, to every organ and tissue. By means of this intricate network, air is carried and supplied almost directly to every particle of living matter. The respiration is not quite

direct with the insects, however, for the oxygen and carbon dioxide must pass through the membranous wall of the air-tube before reaching or leaving the living substance.

In the next and final step, the step taken by the highest forms, the living material is massed, giving rise not only to animals of moderate size, but to the huge creatures that swarm in the seas or walk the earth, like the elephant. With all of these the step in the differentiation of the respiratory mechanism consists in the great perfection of lungs or gills, and in the addition of a complicated circulatory system with a respiratory blood, one of the main purposes being, as the name indicates, to subserve in respiration by carrying to each individual cell in the most remote and hidden part of the body the vital air, and in the same journey removing the poisonous carbon dioxide.

This has been called Indirect Respiration, because the living matter of the body does not take its oxygen directly either from air or water, but is supplied by a middleman, so to speak.

The complicated movements by which water is forced over the gills, or by which the lungs are filled and emptied, and the great currents of blood are maintained—that is, the striking and easily observed phenomena of respiration are thus seen to be only superficial and accessory, only serve as agents by which the real and the essential processes, that go on in silence and obscurity, are made possible.

So far I have attempted to give a brief *résumé* of the views on respiration that have been slowly and laboriously evolved by many generations of physiologists, each adding some new fact or correcting some misconception; and I trust that this brief sketch has recalled to your minds the salient facts in our knowledge of respiration, and that it will give a just perspective, and enable me, if I may be permitted, to briefly describe what I believe to be my own contribution to the ever-accumulating knowledge of this subject.

In 1876-77, Prof. Wilder, who may be said to have inherited his interest in the ganoid fishes directly from his friend and teacher, Agassiz, who first recognized and named the group, was investigating the respiration of the forms Amia and Lepidosteus, common in the great lakes and the western rivers. As his assistant it was my privilege to aid in the researches, and to acquire the spirit and methods as in no other way is it so readily possible, by following out, from the beginning to its close, of an investigation carried on by a master. The results of that investigation were reported to this section in 1877, and formed a part of the proceedings for that year. From that time till the present the problems of respiration in the living world have had an ever increasing fascination for me, and no opportunity has been lost to investigate the subject. The interest was greatly increased by the discovery that a reptile—the soft-shelled turtle—did not conform to the generalizations in all the treatises and compendiums of zoology, which state with the greatest definiteness that all reptiles, without exception, are purely air-breathing, and throughout their whole life obtain their oxygen from the air and never from the water. The American soft-shelled turtles, at least, do not conform to this generalization, but on the contrary, naturally and regularly breathe water like a fish, as well as air like an ordinary reptile, bird, or mammal.

In carrying on the investigation of the respiration of the turtle, there appeared for solution the general problem, which, briefly stated, is as follows: In case an animal breathes both air and water, or more accurately, has both an aërial and an aquatic respiration, like the ganoid fishes, Amia and Lepidosteus, like the soft-shelled turtles, the tadpoles, and many other forms, what part of the respiratory process is subserved by the aqueous and what by the aërial part of the respiration? So far as I am aware this problem had not been previously considered. It was apparently assumed that there were in these fortunate animals two independent mechanisms, both doing precisely the same kind of work—that is, each serving to supply the blood with oxygen and to relieve it of carbon dioxide, as though the other was absent. That was a natural inference, for with many forms the respiration is wholly aquatic, all the oxygen employed being taken from the water, and all the carbon dioxide excreted into it. On the other hand, in the exclusively air-breathing animals, as birds and mammals, the respiration is exclusively aërial.

This natural supposition was followed in the first investigations on the respiration of the soft-shelled turtles, and while it was proved with incontestable certainty that they take oxygen from the water like an ordinary fish—that is, have a true aquatic respiration, in addition to their aërial respiration—there was altogether too much carbon dioxide in the water to be accounted for by the oxygen taken from it. Furthermore, upon analyzing the air from the lungs of a turtle that had been submerged some time the oxygen had nearly all disappeared, and but very little carbon dioxide was found in its place, while, as compared with human respiration, for example, a quantity of carbon dioxide nearly as great as that of the oxygen which had disappeared should have been returned to the lungs. Likewise in Professor Wilder's experiments with Amia, to use his own words: "Rather more than one per cent. of carbon dioxide is found in the normal breath of the Amia, but much more of the oxygen has disappeared than can be accounted for by the amount of carbon dioxide." Everything thus appeared anomalous in this mixed respiration, and instead of a clear, consistent, and intelligible understanding of it, there seemed only confusion and ambiguity. Truly these seemed like "bad experiments."

It became perfectly evident that the first step necessary in clearing the obscurity was to separate completely the two respiratory processes, to see exactly the contribution of each mechanism to the total respiration. But this was no easy thing to do. In the first place, the animal must be confined in a somewhat narrow space in order that air and water, which are known to have been affected by its respiration, may be tested to show the changes produced in it by the respiratory process; in the second place, the water has so great a dissolving power upon carbon dioxide that even if it were breathed out into the air it would be liable to be absorbed by the water. Then some means must be devised to prevent the escape of the gases from the water as their tension becomes changed; and, finally, the animal in the water must be able to reach the air. A diaphragm must be devised which would prevent the passage of gases between the air and the water, and at the same time offer no hindrance to the animal in projecting its head above the water. As a liquid diaphragm must be used, it occurred to me that some oil would serve the purpose, but the oil must be of peculiar nature. It must not allow any gases to pass from air to water, or the reverse; it must not be in the least harmful or irritating to the animal under experimentation, and, finally, it must itself add nothing to either air or water. Olive oil was thought of, and later the liquid paraffins. The latter were found practically impervious to oxygen and fulfilled all the other requirements, but unfortunately they absorb a considerable quantity of carbon dioxide. Pure olive oil was finally settled upon as furnishing the nearest approximation to the perfect diaphragm sought.[1]

The composition of the air being known, and a careful determination of the dissolved gases in the water having been made, the animal was introduced into the jar and the water covered with a layer of olive oil from ten to fifteen millimetres thick. The top of the jar was then vaselined, and a piece of plate-glass pressed down upon it, thus sealing it hermetically. Two tubes penetrate this plate-glass cover, one connecting with the overlying air-chamber and the other extending into the water nearly to the bottom of the jar. As the water and air are limited in quantity, the shorter the time in which the animal remained in the jar the more nearly normal would be the respiratory changes, the experiment was continued only so long—one or two hours—as was found necessary to produce sufficient change in the air and the dissolved gases of the water to render the analyses unmistakable.

Proceeding with the method just described, the results given in the following table were obtained:—

Table of Mixed Respiration, showing the number of cubic centimetres of oxygen removed from air and water, and the amount of carbon dioxide added to the air and the water.

	Oxygen from air	Oxygen from water	Carbon Dioxide to air	Carbon Dioxide to water
Ganoid Fish (*Amia calva*)	65	10	22	53
Tadpoles (*Larval Batrachia*)	70	5	24	51
Soft-shelled Turtle (*Amyda mutica*)	31	8	10	29
Bull Frog (*Rana catesbiana*)	183	4	110	77

NOTE.—The oxygen from both the water and the air, and the carbon dioxide in the air, were determined with exactness in all the experiments; but owing to the failure of some steps in the titration for the carbon dioxide in the water, the figures given for the Amia and the soft-shelled turtle are the calculated results, assuming that the respiratory quotient is one, as that is the relation found by analysis in the other cases.

[1] See Wm. Thörner on the use of olive oil for the prevention of the absorption of carbon dioxide. Repertorium der analytischen Chemie. 1885, pp. 15-17.

It requires but a glance at the figures in this table to see that the aërial differs markedly from the aquatic part of the respiration. Even in the frog, in which the skin forms the only aquatic respiratory organ, the tendency is marked. The law appears to be unmistakably this, viz. that in combined aquatic and aërial respiration, the aërial part is mainly for the supply of oxygen and the aquatic part largely for the excretion of carbon dioxide. This law, which I stated in 1886, has been confirmed by the repetition of old experiments and by many new ones made during the present summer. It is also confirmed by the experiments made on Lepidosteus in a different way by Dr. E. L. Mark, and published in 1890. I therefore feel that this is the expression of a general law in nature.

From the standpoint of evolution we must suppose that all forms originated from aquatic ancestors, ancestors whose only source of oxygen was that dissolved in the water. As the water is everywhere covered with the limitless supply of oxygen in the air, there being 209 parts of oxygen in 1000 parts of air as contrasted with the 6 parts of oxygen dissolved in 1000 parts of water, it is not difficult to conceive that in the infinite years the animals found by necessity and experience that the needed oxygen was more abundant in the overlying air, and that some at least would try more and more to make use of it. And as any thin membrane with a plentiful blood supply may serve as a respiratory organ to supply the blood with oxygen, it is not impossible to suppose that such a membrane, as in the throat, could modify itself little by little with ever-increasing efficiency; and that a part might become especially folded to form a gill and another might become sacular or lung-like to contain air. While I am no believer in the purely mechanical physiology which sees no need of more than physics and chemistry to render possible and explain all the phenomena of life, yet it is patent to every one that, although vital energy is something above and beyond the energies of physics and chemistry, still it makes use of these; and certainly dead matter forms the material from which living is built. So given a living thing, it, in most cases, moves along lines of least, rather than of greatest, resistance; therefore if practically a limitless supply of oxygen may be obtained from the air and only a limited amount from the water, if anything that might serve as a lung is present, most naturally it (the animal) will take the oxygen from the air where it is in greatest abundance and most easily obtained. On the other hand, carbon dioxide is so soluble in water that practically a limitless amount may be excaeted into it; and as it is apparently somewhat easier, other things being equal, for it to pass from the liquid blood to the water than to the air, it seems likewise natural that the gills should serve largely for the excretion of the carbon dioxide into the water. This is the actual condition before us in these, and I believe in all other cases, of mixed or of combined aërial and aquatic respiration. And I believe, as stated above, that it may be laid down as a fundamental law in respiration that wherever both water and air are used with corresponding organs—gills for one and lungs for the other—that the aërial part of the respiration is mainly for the supply of oxygen, and the aquatic part largely for the getting rid of carbon dioxide.

It is not difficult to see in an actual case like that of the Ganoid Fishes (Amia and Lepidosteus) the logical steps in its evolution, by which this most favourable condition has been reached. A condition rendering these fishes capable of living in waters of almost all degrees of purity, and thus giving them a great advantage in the struggle for existence. But what can be said of the soft-shelled turtles, animals belonging to a group in which purely aërial respiration is almost exclusively the rule? Standing alone, this might be exceedingly difficult or impossible of explanation. The Batrachia (frogs, toads, salamanders, &c.) all have gills in their early or larval stage, and most of them develop in the water, and are in the beginning purely aquatic animals. The adults must therefore, in most cases, repair to the water at the spawning season and frequently in laying the eggs they must remain under the water for considerable intervals. Being under the water, and the need of oxygen becoming pressing, there seems to be, by a sort of organic memory, a revival of the knowledge of the way in which respiration was accomplished, when, as larvæ, their natural element was water, and they take water into the mouth and throat. This may be done by as highly a specialized and purely aërial form as the little brown tree-frog (*Hyla pickeringii*) or the yellow spotted salamander (*Amblystoma punctatum*). Another very interesting form, the vermilion-spotted newt (*Diemyctylus*), after two or three years of purely aërial existence goes to the water on reaching maturity and remains there the rest of its life, regularly breathing both by its lungs and by taking water into its mouth and throat. A still more striking example is given by Prof. Cope. The young siren almost entirely loses its gills, and later regains them, becoming again almost completely aquatic in its habits as in the larval stage.

With these examples, which may be seen by any one each recurring year, is it impossible or difficult to conceive that in the struggle for existence the soft-shelled turtles found the scarcity of food, the dangers and hardships on the land greater than those in the water? Or, remaining constantly in the water, and advantageously submerged for most of the time, it gradually reacquired the power of making use of its pharyngeal membrane for obtaining oxygen from the water and excreting carbon dioxide into it as had its remote ancestors. And further, is it not intelligible that with capacious lungs, which it can fill at intervals with air containing so large a supply of oxygen that it, like the other double or mixed breathers, should use its lungs to supply most of the oxygen and its throat to get rid of much of the carbon dioxide?

Indeed it seems to me that if the evolution doctrine is a true expression of the mode of creation, then development may be in any direction that proves advantageous to an organism, even if the development is a reacquirement of long discarded structures and functions.

In closing, may I be permitted to say to the older biologists—to those familiar with the encouragements and inspirations that come with original investigation, that I trust they will pardon what to them is unnecessary personality or excess of detail in this address, for the sake of the younger ones among us, to whom the uphill road of research is less familiar. Judging from my own experience in listening to similar addresses by my honoured predecessors, it is helpful to know, when one is beginning, something of the "dead work," the difficulties and discouragements, as well as the triumphs, in the advancement of science.

MINES AND MINING AT THE CHICAGO EXHIBITION.

THE exhibition of objects relating to mines and mining at the "World's Fair" promises to be one of exceptional interest and importance. The following details about it were given by Mr. George F. Kunz in a paper read before the recent meeting of the American Association for the Advancement of Science:—

The building of mines and mining, which is entirely completed, is 700 feet long and 350 feet wide, at an elevation of 25 feet above the main floor. On both sides is a gallery 60 feet wide, running the entire length of the building. Up to the present time there have applied for space in this building 26 foreign Governments and 36 States, these exhibits to be supplemented by other State and Government exhibits, such as that of Sweden in the Swedish building, the East Indian in the East Indian court, Illinois in their State building, &c.

There will be a scientific collection of all the known elements, and with them a complete collection of all the known alloys of gold, silver, copper, zinc, tin, &c., such as electrum, German silver, Babbits metal, fusible metal, and the thousand and one other, common and rare, used in the arts and industries. In the name of the Lake Superior copper mines, Prof. Alex. Agassiz has promised a complete exposition of ores, rocks, and processes, illustrating the occurrence mining, metallurgy of copper. There is now in preparation a coal collection to contain all varieties of coal, from every known occurrence in the United States. Petroleum will be shown as it never has been at any exhibition. The subject of abrasives of all kinds will form a special exhibit under the charge of Mr. T. Dunkin Paret, who has devoted his entire life to this subject, and is now making a special European trip to enlist the co-operation of foreign manufacturers and investigators to supplement the American exhibit.

The De Beers Mining Company of South Africa, who own and control more than 95 per cent. of the entire diamond output, will make first a full and comprehensive exposition of diamond mining and the original blue stuff, a decomposed peridotite, enclosing carbonaceous shale, the matrix of the diamond, in great quantities. They will show it passing through

the various washing machines, and every process separating the diamond from the matrix, in which exists a percentage of 1 carat 205 milligrams in a load of 1600 pounds. There will be a case containing over 10,000 carats of diamonds of all colours and of the various qualities, with a full series of the associated minerals and rocks. Every stage of the cutting and polishing of the diamond will be represented.

Nearly every mineral dealer in the United States has applied for space, and from the foreign trips and other preparations it is very evident that in the line of cabinet specimens and educational minerals the assembled collections will exceed those of any other exposition in importance.

One of the large gallery halls will contain a reference library for the use of visitors. This it is hoped will be a very comprehensive exposition of the literature of the subject of mines, mining, geology, and mineralogy. This is to be supplemented by historical portraits, documents, and other allied material.

An early history of mining and mining processes will be shown, starting with stone hammers and other aboriginal implements found in the copper mines of Lake Superior and the turquoise mines of New Mexico, the old Mexican Pateo, to the most improved modern methods, and the remarkable sectional and glass models of mines, prepared by eminent mining engineers, used in the great mining lawsuits to prove their arguments.

One of the large corridor rooms in the gallery has been offered to the American Institute of Mining Engineers for their own use as a headquarters during the Exposition. They in turn may extend the courtesy to mechanical and civil engineers, as well as the English, German, French, and other foreign engineers whose hospitality they enjoyed in 1889. There is every reason to believe that at least from 800 to 1000 foreign engineers will visit the Exposition.

If only three-fourths of the promised exhibits are received, and there is every assurance that there will be many more coming, it may be safely said, even now, that the mining, metallurgical, geological, and mineralogical exhibits of the Columbian World's Fair will exceed in scientific importance and in extent the combined exhibits of the Centennial, the 1878, 1889, the Paris and the Vienna Expositions, at least two-fold.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Lord Walsingham, the High Steward of the University, has expressed his wish to give annually for three years a gold medal for the best monograph or essay giving evidence of original research in any subject coming under the cognizance of the Special Board for Biology and Geology. The offer having been accepted and the regulations for the medal having been approved, the Special Board for Biology and Geology give notice that the medal is offered for competition for the second time during the ensuing academical year. The essays are to be sent to the chairman of the Special Board (Prof. Newton, Magdalene College) not later than October 1, 1893.

The regulations for the medal are published in the *Cambridge University Reporter*, No. 908 (November 17, 1891), p. 186.

Sir R. S. Ball, Loundean Professor, will give his inaugural lecture in the Anatomical Theatre on Friday, October 21, at noon.

Dr. Cayley, Sadlerian Professor of Mathematics, resigns his place on the Council of the Senate on October 25.

The Council of the Senate recommend that the University of the Cape of Good Hope should be affiliated to Cambridge, on the same terms as those granted to New Zealand.

LONDON.—Four lectures upon "The Sun in its Relation to the Universe of Stars" will be delivered in Gresham College, at six p.m. on the evenings of October 25, 26, 27, and 28, 1892, by the Rev. Edmund Ledger.

SCIENTIFIC SERIALS.

The Journal of the Royal Agricultural Society of England, 3rd Series, Vol. iii., pt. 3.—Allotments and Small Holdings, by Sir J. B. Lawes and Dr. Gilbert. The authors have collected statistics relating to Allotments and Small Holdings in Great Britain. They point out that "within the present century there has been a great reduction in the number both of owners and of occupiers of farms not exceeding 50 acres in area, such as it seems to be the object of the promoters of the Small Holdings Act of 1892 greatly to increase." After noticing the Rothamstead Allotments they proceed to discuss the conditions essential to the success of small holdings, and they conclude that ordinary rotation farming is much less suitable for small holdings than dairy farming, the production of poultry and eggs, and market gardening when favourable conditions exist; the authors do not believe, however, that the system of small holdings will materially check the influx of agricultural labourers into the towns. This number of the *Journal* also contains a short article by W. H. Hall on Small Holdings in France. Mr. Hall is "convinced that small holders (in England) have a great future before them as soon as they can be educated up to producing such articles as require to be consumed fresh, and will not bear long carriage." This last clause contains the key of the whole matter.—On the Vermin of the Farm, pt. ii., by J. E. Harting. In this paper the author has much to say in defence of the mole (*talpa europæa*), and of the weasel (*mustela vulgaris*); there is little but condemnation, however, for the hedgehog, the stoat, and the polecat; the last-mentioned animal is now hardly known to most people, though the domesticated variety (the ferret) is common.—The Warwick Meeting of 1892, by Dr. Fream, Official Reporter. This report shows the meeting to have been a good average one, except in the attendance of visitors on the last two days. Judge's reports show that in many cases the quality of the exhibits of live stock was far above the average.—Miscellaneous Implements Exhibited at Warwick, by T. H. Thursfield.—The Farm Prize Competition of 1892, by J. B. Ellis.—Among the shorter articles is one deserving of careful attention, entitled New Modes of Disposing of Fruit and Vegetables, by Chas. Whitehead, in which are discussed the "evaporating" and the "canning" of fruit; methods already in use in Queensland are described and discussed with reference to their adoption in this country when prices for fresh fruit are low.—Dr. J. W. Leather contributes a short article upon his method of detecting and estimating "castor-oil seeds in cattle foods." A weighed quantity of the suspected food is digested with hot dilute sulphuric acid (or HCl, about 2 p.c.) for half an hour, washed free from acid, re-digested with a hot dilute solution of caustic soda, washed, and then finally treated with a quantity of bleaching powder. The husks of all seeds other than castor-oil seeds are bleached by this treatment, and any unbleached husks can be picked out and weighed.

Wiedeman's Annalen der Physik und Chemie, No. 9.—The principle of least effect in electrodynamics, by H. von Helmholtz.—On the differences of potential of chains with dry solid electrolytes, by W. Negbaur.—On the reciprocity of electric osmose and flow currents, by U. Saxén.—Resonance phenomena and absorptive capacities of metals for the energy of electric waves, by V. Bjerknes.—Objective presentation of the Hertzian experiments with rays of electric force, by L. Zehnder.—Dispersion and absorption of light according to the electrical theory of light, by D. A. Goldhammer.—On the measurement of high temperatures, by L. Holborn and W. Wien. The apparatus was a modification of Le Chatelier's thermo-element, consisting of a combination of platinum and a platinum-rhodium alloy. This was calibrated by placing it inside the porcelain vessel of an air-thermometer and compairing the readings, different thermo-couples were compared by exposing together in short porcelain tubes, two branches being welded together. The following fusing temperatures were deduced: gold 1072°, silver 968°, copper 1082°.—On the expansion of gases at low pressures, by G. Melander. Working with pressures ranging from 770 to 4mm, and temperatures from 0° to 100°, the gases being kept at constant volume, the supposed law of constant decrease of coefficient of expansion with decreasing pressure was found not to hold good. That of air decreases down to 232mm, where it is 0·003659, and then increases. That of carbon dioxide decreases down to 76mm, after which it increases, whilst that of hydrogen increases steadily.—Specific gravity and heat of fusion of ice, by J. v. Zakrzevski. The apparatus was a very delicate form of Bunsen's ice calorimeter. The specific gravity of ice at −0·701° C. was found to be 0·916710. The cubical coefficient of expansion at that temperature was 0·000077, which gives for the sp. gr. of ice at 0° C the value 0·916660.—On the theoretical conceptions of Georg Simon Ohm, by K. Von der Mühll.—Variation of the specific volume of sulphur with the temperature, by M. Toepler.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"Thermal Radiation in Absolute Measure." By Dr. J. T. Bottomley, M.A., F.R.S.

The paper contains an account of an experimental investigation by the author in continuation of researches on the same subject which have been already published (Roy. Soc. Proc., 1884, and Phil. Trans., 1887). In the earlier experiments metallic wires heated by an electric current were used. The loss of heat from a heated body, however, depends to some extent on the form and dimensions of the body; and it seemed important to experiment on the loss of heat from bodies differing in form from the wires already used, and larger in dimensions.

Accordingly, two copper globes used by Mr. D. Macfarlane in 1872 (Roy. Soc. Proc., 1872, p. 93) were employed for a new series of experiments.

After preliminary experiments (using the same enclosure which Macfarlane employed and with the surfaces of Macfarlane's globes prepared in four different ways) new apparatus was constructed. The object was to experiment both with full air pressure and with different amounts of exhaustion of the air, and Macfarlane's enclosure is unsuitable for this purpose.

In the arrangement adopted, the heated globes were hung at the centre of a hollow metallic sphere, which was connected with the Sprengel pump and surrounded with cold water, and were allowed to cool. The temperature of the cooling globe was read off at equal intervals of time by means of a thermo-electric junction; and from these readings the absolute loss of heat per unit of cooling surface, per unit difference of temperatures of surface and surroundings, per unit of time, is calculated.

The details of the apparatus and method of experimenting are given in the paper. It is enough to say here that the globes were used with their surfaces in two different conditions:—(1) Thinly coated with lamp-black, and (2) silvered and brightly polished; and in both conditions the absolute loss of heat, both in air and in vacuum more or less complete, was determined. The tables and curves attached to the paper give the details of the results.

To quote one or two examples:—With the sooted surface a total loss of heat by convection and radiation of $3{\cdot}42 \times 10^{-4}$ c.g.s. units per square centimetre, per second, per 1° C. of difference of temperatures of globe and surroundings, was observed with a difference of temperatures of 100° C., and with the surroundings at about 14° C. Under similar circumstances the radiation in vacuum of ½M (half-a-millionth of atmospheric pressure of non-collapsible gas) was about $1{\cdot}40 \times 10^{-4}$.

Taking a silvered and brightly-polished surface under the same circumstances, the loss in full air was $2{\cdot}30 \times 10^{-4}$ c.g.s.; and with the highest vacuum and brightest polish obtained, it was reduced $1{\cdot}80 \times 10^{-5}$, with in this case a difference of temperatures of 180° C. The loss with 100° C. difference would be considerably less, but is not known experimentally at present.

The author returns thanks to Mr. James H. Gray, M.A., B.Sc., for excellent assistance given; and expresses himself most deeply indebted, both for assistance in experimenting and calculating of the results, and for most valuable and ingenious aid of various kinds during the course of this work, to his friend Mr. A. Tanakadate, now Professor in Tokio, Japan.

Entomological Society, October 5. Henry John Elwes, vice-president, in the chair.—Mr. C. O. Waterhouse exhibited a specimen of *Latridius nodifer* feeding on a fungus, *Trichosporium roseum*.—Mr. McLachlan, F.R.S., exhibited a male specimen of *Elenchus tenuicornis*, Kirby, taken by the Rev. A. E. Eaton, on August 22 last, at Stoney Stoke, near Shepton Montague, Somerset, and described by him in the *Entomologist's Monthly Magazine*, October 1892, pp. 250-253. Mr. McLachlan stated that another specimen of this species had been caught about the same date in Claygate Lane, near Surbiton, by Mr. Edward Saunders, who discovered that it was parasitic on a homopterous insect of the genus *Liburnia*, and had also described it in the *Entomologist's Monthly Magazine*.—Mr. J. M. Adye exhibited, for Mr. McRae, a large collection of *Colias edusa*, *C. edusa* var. *helice*, and *C. hyale*, all taken in the course of five days' collecting in the neighbourhood of Bournemouth and Christchurch, Hants. There were twenty-six specimens of *helice*, some of which were remarkable both in size and colour. He stated that Mr. McRae estimated the proportion of the variety *helice* to the type of the females as one in fifty. Mr. Adye also exhibited two specimens of *Deiopeia pulchella*, recently taken near Christchurch. Mr. Hanbury, Mr. Jenner-Weir, and Mr. Merrifield commented on the interesting nature of the exhibition, and on the recent extraordinary abundance of *C. edusa* and the var. *helice*, which was probably not exceeded in 1877.—Mr. Dallas-Beeching exhibited four specimens of *Plusia moneta*, lately taken in the neighbourhood of Tunbridge Wells.—Mr. H. Goss exhibited, for Mr. Gervase F. Mathew, two *Plusia moneta* and their cocoons, which were found at Frinsted, Kent, on September 3 last. It was stated that Mr. Mathew had found seven cocoons on the under side of the leaves of monkshood, but that the imagos had emerged from five of them.—Mr. Rye exhibited a specimen of *Zygæna filipendulæ* var. *chrysanthemi*, and two varieties of *Arctia villica*, taken at Lancing, Sussex; also varieties of *Coccinella ocellata* and *C. oblongoguttata* from Oxshott.—Mr. A. H. Jones exhibited specimens of *Argynnis pales* var. *isis*, and var. *arsilache*, the females of which showed a tendency to melanism, recently taken in the Upper Engadine; also melanic forms of *Erebia melampus*, and a specimen of *Erebia nerine*.—Mr. Elwes exhibited specimens of typical *Erebia melas*, taken by himself in the Western Tyrol, on July 25 last, at an elevation of 7000 feet: also specimens of the same species from Hungary, Greece, and the Eastern and Central Pyrenees. He stated that the absence of this species from the Alps, which had seemed to be such a curious fact in geographical distribution, had been first disproved by Mrs. Nicholl, who discovered it at Campiglio two years ago. He also exhibited fresh specimens of *Erebia nerine*, taken at Riva, on the lake of Garda, at an elevation of about 500 feet; also specimens of the same species, taken at the same time, at an elevation of about 5000 feet, in cool forest glades; and remarked that the great difference of elevation and climate did not appear to have produced any appreciable variation in this species.—Mr. G. T. Porritt exhibited two varieties of *Abraxas grossulariata*, bred during the past summer from York larvæ. Also a curious Noctua taken on the sandhills at St. Anne's-on-Sea on August 20 last, and concerning which a difference of opinion existed as to whether it was a melanic form of *Agrotis cursoria* or of *Caradrina cubicularis*. Also a small dark form of *Orgyia antiqua*, which had occurred in some numbers at Longridge, near Preston.—Mr. A. Eland-Shaw exhibited a specimen of *Mecostethus grossus*, Linn., taken lately at Irstead, in the Norfolk-broad district. He stated that this was the first recorded capture of this species in Britain since 1884.—Mr. C. G. Barrett exhibited a specimen of *Syricthus alveus*, caught in Norfolk about the year 1860; a beautiful variety of *Argynnis euphrosyne*, caught this year near Godalming; and a series of varieties of *Ennomos angularia*, bred from a female taken at Nunhead.—Mr. P. Crowley exhibited a specimen of *Zygæna filipendulæ* var. *chrysanthemi*, taken last August at Riddlesdown, near Croydon.—Lord Walsingham, F.R.S., sent for exhibition several specimens of larvæ of *Sphinx pinastri*, preserved by himself, which were intended for presentation to the British Museum. The larvæ had been sent to him by Lord Rendlesham, who obtained them from ova laid by a female captured in Suffolk last August.—M. de Nicéville communicated a paper entitled "On the variation of some Indian Euplœas of the subgenus *Stictoplœa*"; and Captain E. Y. Watson exhibited, on behalf of M. de Nicéville, the specimens referred to in this paper. Colonel Swinhoe, Mr. Hampson, Mr. E. B. Poulton, F.R.S., and the chairman took part in the discussion which ensued.—Mr. W. Bateson read a paper entitled "On the Variation in the Colours of Cocoons and Pupæ of Lepidoptera; further Experiments."—Mr. Poulton read a paper entitled "Further Experiments upon the Colour-relation between certain Lepidoptera and their Surroundings."—Miss Lilian J. Gould read a paper entitled "Experiments on the Colour-relation between certain Lepidopterous larvæ and their surroundings, together with observations on Lepidopterous larvæ." A long discussion ensued, in which Mr. Jenner Weir, Dr. Sharp, F.R.S., Mr. Merrifield, Mr. Poulton, and the chairman took part.

PARIS.

Academy of Sciences, October 10. M. Duchartre in the chair. M. Emile Picard presented to the Academy the second volume of his "Traité d'analyse."—The University of Padua invited representatives of the Academy at the forthcoming tercentenary celebration of Galileo's accession to his chair at that University.—A decisive blow to the theory of centripetal and ascending motion in cyclones, by M. H. Faye.—The move-

ments of the heart, studied by chronophotography, by M. Marey. The heart of a tortoise was removed and mounted so that a funnel led into an auricle, and a bent tube out of the ventricle and upwards to the mouth of the funnel. The funnel was filled with defibrinated blood, which passed into the auricle and thence into the ventricle. When the latter was full, an automatic systole projected the blood upwards through the tube and back into the funnel. This process was repeated for several hours after death. It was more minutely studied by taking a series of instantaneous photographs in rapid succession (reproduced), which show the details of the process with great accuracy. For actinic purposes, the heart was painted white with water-colour. The hypothesis of an active diastole of the ventricle was proved to be unfounded.—The inhibitory phenomena of the nervous shock, by M. H. Roger.—On the transformation of the equations of Lagrange, by M. Paul Painlevé.—On a class of curves and surfaces, by M. A. Pellet. On the motion of a thread in space, by M. G. Floquet.—On internal reflection in crystals, by M. Bernard Brunhes.—A new method of preparation and photometry of the phosphorescent sulphide of zinc, by M. Charles Henry. It is possible to obtain several pounds at a time of a fine phosphorescent zinc sulphide by the following process: Add ammonia to a perfectly neutral solution of pure zinc chloride; redissolve the precipitate in an excess of ammonia; precipitate completely, but without the slightest excess, the ammoniacal oxide of zinc by sulphuretted hydrogen; heat to a white heat in a crucible of refractory earth placed inside a graphite crucible, after having well washed and dried the amorphous sulphide to the exclusion of all impurities. By Mascart's photometer, the intensity of light emitted by a sample of the sulphide in candle-metres after saturation was 0·000215. But this value is probably too small. —On the antimonites of pyrogallol, by MM. H. Causse and C. Bayard.—On the tartaric ethers, by M. P. Freundler.—Volumetric determination of the alkaloids, by M. L. Barthe.—On a new method of brick manufacture, used in certain parts of Central Asia, by M. Edouard Blanc. This mode of manufacture is practised by the tribes in Western Mongolia, on the frontier of Siberia. The extremes of temperature render a brick of great durability a necessity of life. This is attained by the use of steam. The oven is cylindrical and surmounted by a hemispherical cap, which is kept open for the first three days. The bricks, about 7000 at a time, are baked by means of a fire fed by about 7000 kgr. of an annual ligneous plant, the *Alhagi Camelorum*. On the third day, the opening is closed with felt, which is kept constantly wetted, so that the bricks are enclosed in a steam bath, while kept at a red heat. Under these circumstances, some novel chemical reactions appear to take place. The bricks, red after the first period, appear dark grey after the second part of the process. Their structure appears porous; they become sonorous and acquire a great hardness. They show a striking resemblance to certain trachytes. Made from the same clay as our bricks, they resist weathering very much better, and have an extraordinary hardness and cohesion.—A process for testing the purity of coprah oils and palm oils, by M. Ernest Milliau.—On the part played by spermine in intra-organic oxidations, by M. Alexandre Poehl.—On the respiration, transpiration, and dry weight of leaves developed in sunlight and in the shade, by M. L. Geneau de Lamarlière.—On the structure of the assimilating tissue of the branches in Mediterranean plants, by M. William Russell.—Experimental study of the action of the humidity of the soil on the structure of branches and leaves, by M. Auguste Oger.—Contributions to the stratigraphy of the Pyrenees, by MM. Roussel and De Grossouvre.—On some bombs of Etna, from the eruptions of 1886 and 1892, by MM. L. Duparc and L. Mrazec.—Meteoric iron recently fallen at Hassi Iekna, in Algiers, by M. Stanislas Meunier. —Oceanographic observations relating to the basin of Arcachon (Gironde), by M. J. Thoulet.—Vegetation of the lakes of the Jura mountains, by M. G. Rambault Ant. Magnin.—M. Bischoffsheim, on behalf of Prof. Weineck, Director of the Prague Observatory, presented a photograph of the lunar crater Vendelinus.

DIARY OF SOCIETIES.

LONDON.

SUNDAY, OCTOBER 23.

SUNDAY LECTURE SOCIETY, at 4.—The Distribution of Animals and what it Teaches (with Oxy-hydrogen Lantern Illustrations): Dr. Andrew Wilson.

TUESDAY, OCTOBER 25.

MINERALOGICAL SOCIETY.—Anniversary Meeting.—Council Report.—On Crystallized Zirconia (Baddeleyite), a New Mineral Species from Ceylon: L. Fletcher, F.R.S.—Preliminary Note on Xanthoconite and Rittingerite: H. A. Miers and G. T. Prior.—A Locality of Cerium Minerals in Cornwall; H. A. Miers.—On Gypsum from Herne Bay: F. Rutley.

FRIDAY, OCTOBER 28.

PHYSICAL SOCIETY, at 5.—Discussion of Mr. Williams's Paper on the Dimensions of Physical Quantities.—Dicussion of Mr. Sutherland's Paper on the Laws of Molecular Force, to include Papers by Dr. Young and Mr. Thomas on the Determination of Critical Density, Critical Volume, and Boiling Points.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Framework of Chemistry, Part 1; W. M. Williams (Bell).—A German Science Reader; F. Jones (Percival).—Chemical Lecture Experiments: G. S. Newth (Longmans).—Outlines of Psychology, new edition: Dr. J. Sully (Longmans).—Animals' Rights: H. S. Salt (Bell).—University College of North Wales Calendar for the Year 1892–93 (Manchester, Cornish).—The Climate of Rome and the Roman Malaria: Prof. Tommasi-Crudeli, translated (Churchill).—The Fauna of Liverpool Bay, Report 3 (Liverpool, Dobb).—Atomic Consciousness (Whimple, Harris).—Geographische und Naturwissenschaftliche Abhandlungen, I.: Dr. J. Rein (Leipzig, Engelmann).—Metal-Colouring and Bronzing: A. H. Hiorns (Macmillan).—The Telephotographic Lens: T. R. Dallmeyer (Dallmeyer).—The Geological and Natural History Survey of Minnesota: N. H. Winchell (Minnesota).—Brachiopoden der Alpinen Trias, Nachtrag I.: A Bittner (Wien).—Atlas der Völkerkunde: Dr. G. Gerland (Gotha, J. Perthes).—British Fungus Flora, vol. i.: G. Massee (Bell).

PAMPHLETS.—Rutherfurd Photographic Measures of the Stars about β Cygni: H. Jacoby (New York).—Ueber die Einseitigkeit der Herrschenden Krafttheorie: Dr. N. von Seeland (Leipzig, Pfeffer).—Weitere Untersuchungen über die Tägliche Oscillation des Barometers: J. Hann (Wien).

SERIALS.—Internationales Archiv für Ethnographie, Band 5, Heft 4 (Kegan Paul).—Annals of Scottish Natural History, October (Edinburgh, Douglas).—Palestine Exploration Fund Quarterly Statement, October (Watt).—Transactions of the Leeds Naturalists' Club, &c., 1890, vol. 1 (Leeds).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 1891-92, vol. 5, No. 2 (Manchester).—Notes from the Leyden Museum, vol. xiv. Nos. 1 and 2 (Leyden, Brill).—Morphologisches Jahrbuch, 18 Band, 4 Heft (Williams and Norgate).—Botanische Jahrbucher für Systematik, Pflanzengeschichte und Pflanzengeographie Sechzehnter Band, 2 Heft; Fünfzehnter Band, 4 Heft (Williams and Norgate). —Journal of the Royal Statistical Society, September (Stanford).—Mind, October (Williams and Norgate).—The Asclepiad, No. 35, vol. 9 (Longmans).—Medical Magazine, October (Southwood).—Jahrbuch der k.k. geologischen Reichsanstalt, Jahrg. 1892. xlii. Band, 1 Heft (Wien).—Bulletin of the New York Mathematical Society, vol. 2, No. 1 (New York).

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